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UNIVERSITY OF ALBERTA

NATIVE AMERICAN ORIGINS: A HOLISTIC SYNTHESIS

BY

JOHN ALEXANDER ALSOSZATAI-PETHEO ©

A thesis submitted to the Faculty of Graduate Studies and Research in partial fulfillment of the requirements for the degree of Doctor of Philosophy.

DEPARTMENT OF ANTHROPOLOGY

Edmonton, Alberta
Fall 1992

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"...it is indeed fortunate that we do not settle scientific questions by the votes of experts."

— C. Vance Haynes 1973:1372 —

"An empirical science is hypothesis testing. Data does not speak for itself [sic]. I have been in rooms with data and listened very carefully. The data never said a word."

— Milford H. Wolpoff 1975:15 —

"Strong inference redirects a man to problem-orientation, but it requires him to be willing repeatedly to put aside his last methods and teach himself new ones."

— John R. Platt 1964:351 —

In memory and recognition of those intrepid early pioneers, the timelessly remote ancestors of Native Americans everywhere, the first Americans.

ABSTRACT

This work presents a departure from the traditional archaeological paradigm which has yielded an impasse in the study of the Pleistocene peopling and early prehistory of the Americas. By adopting a broad and holistic approach to the definition of relevant phenomena it is argued that extra-archaeological data and conclusions, independently obtained from other disciplines and subdisciplines, and internally consistent with the method and theory of those disciplines, should be incorporated into reconstructions of the early prehistory of the Americas. This approach, modeled in part on the multidisciplinary approach employed in paleoanthropology, is used in this work to generate a theoretical model and related hypotheses independent from the Pleistocene archaeological record of the Americas. The seventh chapter presents a comparison between the predictions of the theoretical model and a short sampling of Pleistocene American sites. The last chapter provides a closer examination of two early sites, while suggesting alternative methodological approaches. It is hoped that the adoption of the alternative approach presented in this work will provide a solution to the archaeological impasse which has characterized the field in the past.

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Chapter One

INTRODUCTION

This work concerns the initial peopling and early prehistory of the Americas—that which I shall call "Proto-American prehistory." Whereas the topic can be simply stated, one immediately encounters inherent difficulties incumbent upon any further attempts to define the problem. These difficulties can be traced in part to chance historical events, and in part to the associated assumptions and problem-boundary definitions which have guided past research into the problem. The following general observations are offered at this point because this work proposes to examine additional, and fundamental alternative theoretical approaches which are not normally addressed by typical works whose focus tends to be narrower.

The perspective offered in this work holds, in part, that the manner and limits by which one defines the scope of what constitutes the legitimate domain of Proto-American prehistory is in need of revision. In this context, "revision" is not equated with a call to abandon past fundamental gains realized by the field, but with a call to re-examine fundamental issues, and to broaden research objectives and methods.

Historical reviews of American archaeology's concern with fundamental issues relating to the initial peopling of the Americas point to a long, and complicated history (cf. Wilmsen 1965; Willey and Sabloff 1980). Thus, unless one chooses deliberately to review the full history of related developments and ideas over the last 500 years, one must pick an arbitrary starting point which invariably will satisfy some, but not others. It is not my intent to provide a historical review of events at this point. Instead, I hope to offer some brief observations which might clarify the context of the theoretical orientation of this work. This orientation, in part, has been prompted by historically recent critiques of the field which have led me to the conclusion that a fundamental reevaluation of our approach is necessary.

Wilmsen's (1965) "outline of early man studies" was published at a critical juncture in these studies. During the preceding fifteen years American archaeology saw the introduction of a variety of technical aids from the natural sciences including, among others, the widespread use of radiocarbon dating. Willey and Sabloff (1980:155) state that

> ...it is often felt that, with the advent of the 1950s, American archaeology suddenly added a scientific tool to its regular equipment and became a radically changed discipline. This, however, was not quite what happened. Although new dating techniques, and particularly radiocarbon dating, did have an immediate impact on American archaeology, techniques from other disciplines had only just begun to affect archaeology in the New World; and, although the archaeologist

> began to use a host of new scientific aids, he was not prepared conceptually to use them in holistic combination or in ways that would affect significantly the kinds of data he was unearthing [emphasis added].

Still another set of authors (Moore and Keene 1983:4-5) have expressed the historical trends in modern American archaeology as follows.

> In reviewing the advances within archaeology, the Law of the Hammer comes to mind. Stated in its most elementary form, the law predicts that given a hammer, a young child will find that the world is poundable... The image of the hammer provides a robust metaphor for much of contemporary archaeology. Methods are tools. They are developed for specific tasks to produce specific types of observations. But methods, like tools, can be abused. The most obvious form of abuse involves using methods not because they fit the task at hand, but because they are methods we know can easily apply. The Law of the Hammer suggests that, although the methods we use are often appropriate to the task, too frequently a given method is used simply because it is the tool currently in hand. In these cases the pounding has produced more than a little noise, yet little anthropological understanding of prehistory has been constructed...

Although Moore and Keene (1983) are clearly discussing the incorporation and use of new methods in a broader sense than Willey and Sabloff (1980), the caveat of the "Law of the Hammer" applies equally well to the introduction of "new scientific tools" to the discipline. Willey and Sabloff's (1980) observation that archaeologists were "not prepared conceptually to use them in holistic combination" (see above), agrees well with the analysis provided by Moore and Keene (1983).

Wilmsen's (1965) historical review was published at a time when two contrasting syntheses in "early man studies" had been published (Krieger 1964, and Haynes 1964). These two, and related works, set the stage, and the tenor of the "debate" which has characterized the field during the subsequent decades. I would also argue that much of this debate, with its "proponents" and "opponents," has been largely method-driven to the detriment of the growth of robust theoretical constructs. As such, these historical trends may be interpreted as one expression of the "Law of the Hammer."

Indeed, much of Dincauze's (1984) review of the state of the "debate" revolves around the ills of a retarded theoretical foundation which at one point (Dincauze 1984:278) describes as "inchoate." Although Dincauze's (1984) comments suggest that the problems associated with a week theoretical base represent the unilateral shortcoming of the "Pre-Clovis" side of the controversy, the fact remains that neither side of the debate has marshalled a theoretical model of broad scope, and sufficient strength to successfully integrate all of the archaeological data, or to convince the other side of the validity of its position. Instead, it seems that over the last few decades, research into Proto-American prehistory has become increasingly focused on the solution of specific technical issues. This approach has been made possible by the continued adoption of increasingly sophisticated methods to solve field, and laboratory problems.

Advances which permit researchers to focus on increasingly narrower technical issues may well represent normal, and even desirable developments. The adoption of these advances only becomes a problem when they are not accompanied by equivalent advances in the theoretical framework employed by the field. Failure to develop and interpret empirical data through appropriate theoretical constructs has led to the current, and resultant differences of opinion among researchers in Proto-American prehistory which Dincauze (1984) has characterized as an "impasse." Under these conditions, theory does not anticipate new data, and new explanations are driven by the acquisition of new empirical discoveries leading to the generation of new empirical generalizations.

Added to these problems, a number of researchers on both sides of the debate (e.g., Carter, Simpson, Martin, and Lynch) have adopted increasingly more extreme positions. Extreme positions in any debate can become useful sources of warning to the field. In this "debate" the "Law of the Hammer" may explain the reductionist approach adopted by some (e.g., Payen 1982); while, in like vein, both sides of the "debate" might profit by heeding Cole's (1980) observation that "when a search for early man deals only with dating, it ignores the fact that dating is an interpretive tool rather than a goal in itself."

Jeremy Sabloff, a self-described "outsider," has provided some additional observations on the state of the "debate." Parenthetically, and within the context of the present discussion, it may be argued that given the extreme polarization of the field, an "outsider" may well offer a more insightful and objective analysis of the problem than an "insider," however "expert," who, by contrast, may be too closely associated with either side of the "impasse." In Sabloff's words,

> Essentially, it appears to this outsider as if most of the research into Ice-Age origins is reactive. Scholars go out and make new discoveries, new interpretations are made on the basis of discoveries, and existing origins models are revised to accommodate these new interpretations. Those who disagree reject the revisions and reassert the correctness of the status quo. Relatively little progress is made in breaking away from the older models and in formulating new, testable models of broad scope (Sabloff 1988:vii).

The process described by Sabloff (above) most clearly results in what are called empirical generalizations. Empirical generalizations proceed from specific instances, by inductive extension, to whole classes of phenomena. The strength of empirical generalizations comes from concrete examples. At the same time, lacking detailed explanatory power, empirical generalizations are limited to consideration of those phenomena which constitute its inductive base, or to those which can be added to its data base by analogy. A principal alternative to the use of empirical generalizations is the formulation, and use of explicit theoretical models.

Theoretical models are similar to empirical generalizations in that they do not arise in the absence of a broad empirical experiential base. However, unlike inductive generalizations, theoretical models do not proceed as automatic or obligatory conclusions from an inductive data base. Theoretical models represent a unique human creation which trascends its data base while seeking to provide a fundamental, and comprehensive explanation of phenomena. While an empirical generalization is bound by its data base, the explanations proposed by a theoretical model transcend these bounds by providing explanatory extensions tied to other classes of observations, and other data.

The formulation of any theoretical model requires the explanation of logical steps, and reference to underlying principles. Once a new theoretical model has been internalized, the details of empirical data of everyday experience may not be significantly different; but the perceived relationships, and our perceptions of the messages contained in the data may forever be different. Thus, for example, Burke (1985:11) notes that:

> Somebody once observed to the eminent philosopher Wittgenstein how stupid medieval Europeans living before the time of Copernicus must have been that they could have looked at the sky and thought that the sun was circling the earth. Surely a modicum of astronomical good sense would have told them that the reverse was true. Wittgenstein is said to have replied: 'I agree. But I wonder what it would have looked like if the sun had been circling the earth.'
>
> The point is that it would look exactly the same...

The proposition of a new theoretical model is often associated with a more parsimonious explanation which successfully incorporates all data, including those data which were previously viewed as anomalous (Kuhn 1970:67-69). Yet despite their advantages, theoretical models represent only a first step toward problem resolution.

The model output—that is, the specific predictions of a model—are dependent on the quality of the specific data from which the model output was derived. This limitation applies equally, regardless of the classes of phenomena predicted by the model output. Furthermore, any single data class may contain systematic errors or biases which may yield apparent anomalies in the model output when tested against a class of phenomena which the model output intends to predict.

The existence of systematic errors or a systematic bias in the data base from which a model output is generated may not be obvious, and may be very difficult to isolate when the model output only addresses a single class of phenomena (e.g., archaeological data). Instead, it is preferable to utilize more complex models which are capable of integrating two or more independent data bases. The integration of independent data bases can then be used as a set of reciprocal controls to isolate, and (hopefully) eliminate biases, while improving the reliability of the model output.

Past inductive generalizations in "early man" or "Paleo-Indian" studies have yielded a variety of broad culture-historical reconstructions. However, in reviewing these efforts, Willey and Sabloff (1980:212) make some additional recommendations.

> Interest in and debate about the broad cultural-historical questions of New World Early Man studies continue more vigorously than ever before. Who were the earliest inhabitants of the Americas? What was their relationship to Asiatic forebears? What is the story of their complex development in the New World? Such questions set the general bounds of this sector of American archaeology, but archaeologists will be in a better position to answer these questions if they deal with other questions, about the structures and functions of the societies and cultures they are studying.

The call to deal with issues related to the structures, and functions of societies, and cultures is a clear reference to culture-processual concerns. These concerns, in turn, may require either the application of analogies based on the "direct historical approach" (cf. Sharer and Ashmore 1979:53-54)—an unlikely choice given the antiquity of the cultural events under study—or the generation of explicit culture-processual theoretical frameworks of much broader scope. The third chapter of this work provides an initial step towards the generation of such a broad culture-processual framework.

As I have tried to indicate throughout the preceding discussions, the topics related to the initial peopling and early prehistory of the Americas present a complex challenge which have effectively polarized the field. Unfortunately, the lines of division which separate different groups of researchers may not provide the best contrasts leading towards eventual problem resolution. In this work I have tried to define some new contrasting concepts and approaches which might ultimately be more effective in resolving the "impasse" which has characterized the "debate" in the field.

One of the principal contrasts introduced in this work points to a need to generate, and adopt a broad theoretical framework, rather than relying on inductive generalizations for cultural reconstructions. The fundamental approach advocated in this work also draws on four basic premises. These premises include:

1) A recognition of anthropology's long-standing commitment to the solution of complex theoretical problems from a holistic, biocultural perspective.

2) Closely allied to the first premise is the acceptance of the premise that the initial peopling of the Americas represents a series of interdependent events characterized by multiple consequences, aspects, or facets which normally fall within the domain of anthropology, and other related disciplines, and within the domains of more than one subdiscipline of anthropology.

3) Another fundamental premise is that the current archaeological impasse may be due to some form of temporary errors or biases within

the subdiscipline of archaeology; and

4) That these errors or biases may be clarified by the adoption of a model whose model output draws on independent, multiple, and relevant (as defined by the first two premises) data bases not derived from the early archaeological record of the Americas; and against which, ultimately, the totality of the archaeological data base, and interpretations of Proto-American prehistory may be compared.

These four premises encapsulate the intent, and basic theoretical orientation of the model employed in this work. Subsequent chapters will develop a variety of themes, while drawing on a wide range of data, and methods from various disciplines and subdisciplines as they relate to Proto-American prehistory.

The second chapter introduces in greater detail the nature of the problem, and the intended methods and goals of the present work. The third and fourth chapters contain discussions of a broad range of data sets (exclusive of the early archaeological record of the Americas) which the model uses to yield the model outputs presented in chapters five, and six. Chapter seven draws exclusively on primary archaeological sources in order to provide a brief basic comparison of the original archaeological data of Proto-American prehistory to the predictions found in the model output. Finally, chapter eight examines in greater detail two case studies; pointing towards specific theoretical, and methodological approaches applicable to future archaeological work in Proto-American prehistory.

At the beginning of this chapter I introduced the term "Proto-American prehistory" as the initial peopling, and early prehistory of the Americas. At this point I would like to further stipulate that I believe that the topic should be viewed holistically, as I have tried to define it through the premises listed above. Unfortunately, most "insiders" associated with the problem have tended to define, and approach the problem strictly from an archaeological perspective. I believe that this perspective is partly if not largely responsible for the difficulties which the field has encountered in the past. Hopefully this work will enable us to redefine the problem, its scope, its methods, and its goals in such a way that we may resolve the impasse and allow us to pursue more fruitful and relevant issues.

Chapter Two

STATEMENT OF THE PROBLEM

The discussions presented in the preceding chapter highlighted the polarization of the field; a situation which Dincauze (1984) has characterized as a "stalemate" or "impasse" between the "proponents," and "opponents" of the the Pleistocene "pre-Clovis" occupation of the Americas. Dincauze's (1984) use of logic to highlight fallacies in argumentation styles could also be used to illustrate one of the basic problems embedded in the current "impasse."

The proposition, that the Americas were initially peopled (approximately 12,000 years ago) by an influx of groups (from Asia) who left the archaeological record of the Clovis sites, can be equated with the statement that no people were present in the Americas prior to (about) 12,000 years ago. In formal logic, this second statement represents a universal negative proposition (cf. Copi 1982:179). If we also accept the proposition that science proceeds by disproofs (cf. Popper 1959), then we can propose that this universal negative proposition can be falsified by the discovery of one or more American sites which predate the maximum known age of Clovis sites. The fact that the "debate" continues, despite numerous American pre-Clovis sites which have been reported in the literature (cf. summaries provided by Krieger 1964; Willey 1966; MacNeish 1976; and Bryan 1965, and 1991), clearly indicates that the nature of the "debate" can not be reduced to simple logical proofs or disproofs. Unfortunately, this last conclusion has not always been clear to the participants in this "debate." In fact, Dincauze (1984:282) states that

> There exists no 'school' of Pre-Clovis advocates. There is no consistent point of view. There is no widespread agreement about the critical sites, acceptable age ranges, or the theoretical context within which observations can be defined as data. Refusing to stand together, the Pre-Clovis proponents challenge the skeptics to deal with each of them separately—a formidable and thankless task that has been undertaken by only the dauntless few.

In one sense one could argue that there is no need for a "school of Pre-Clovis advocates" in order to test the validity of the proposition that no people were present in the Americas prior to (about) 12,000 years ago. On the other hand, Dincauze is also highlighting the lack of "widespread agreement about the critical sites, acceptable age ranges, or the theoretical context within which observations can be defined as data" among various "proponents" of the pre-Clovis occupation of the Americas.

References to theory or "theoretical context" at this point appear to be misplaced. The theory or theoretical context, within which the data from these, and all other sites (including non-Pleistocene age sites) in the Americas is evaluated, must meet the established standards of prehistoric archaeology. On the other hand, when one examines closely the different interpretations of individual sites discussed by Dincauze (1984), or the contrasting details of the culture-historical reconstructions offered by various authors (e.g., Krieger 1964; MacNeish 1976; and Bryan 1965, 1969, 1978, and 1991), it becomes obvious that the differences originate as typical products of inductive generalizations.

It is wholly inappropriate to suggest that each side of the "debate" should be required to develop its own unique archaeological "theory" (such as the "theory" of the archaeology of pre-Clovis sites). Presumably, Dincauze's (1984) comments were not offered in this vein.

Theory or theoretical context may also refer to specific method-oriented "lower-level theory" (cf. Willey and Sabloff 1980:249-250). In the face of new data, one common approach employed to "reassert the correctness of the status quo" (Sabloff 1988:vii) has been to deny the admissibility of data as evidence on various technical, and methodological grounds (cf. Payen 1982). I feel that this is a dangerous pursuit which invites extreme reductionism, while ignoring the rich theoretical heritage of the discipline. If we are to define "theory" as equivalent to inductive generalizations, or if we are to substitute narrowly-focused "lower-level" methodological theory for genuine theoretical models, we run the risk of reaching an ever-growing number of paradoxes, and "impasses" which are direct artifacts of our orientation.

Rather than accepting these interpretations, it can be argued that Dincauze's reference to the "theoretical context within which observations can be defined as data" appears to be aimed at a different concern. In effect, our definition, and perceptions of the units of knowledge which we call "facts" or "evidence" are inherently dependent upon the theoretical assumptions or paradigmatic approach which we use to gather the data to be marshalled as "facts" (Latt 1987:3). In the case of the "debate" surrounding the earliest prehistory of the Americas, one of the fundamental components of the "debate" has been the acceptance of the premise that the solution to the "impasse" lies in the direction of increasingly narrower orientations towards problem resolution. The theoretical model presented in this work takes a different approach.

2.1 DEFINING THE SCOPE OF THE PROBLEM

Beyond geographical, and temporal boundaries, it becomes clear that the details of Proto-American prehistory are inextricably tied to complex issues like the rise, and spread of anatomically modern humans; the dynamics of population genetics; the rise of genetic variants, and population isolates; the dynamics of cultural adaptation, and culture change; and the relationships of peoples, cultures, and demographic, and ecologic variables in small-scale societies over thousands of years throughout an area equivalent to nearly thirty percent of the habitable land surface of the earth.

This is by no means an exhaustive list of the relevant topics which could be pursued under the general heading of Proto-American prehistory. Significantly, these topics are tied to each other, so that an increase in our understanding of any of them will shed light on the others. Clearly, then, the initial peopling, and early prehistory of the Americas is an event or class of events which should be viewed as a single topic to be approached with a holistic perspective.

The present work takes such a holistic approach. The focus of concern ties all these topics together, not only incorporating but integrating information from the subfields of anthropology, while incorporating perspectives, and information derived from the natural sciences. This concept is not to deny that each line of reasoning should be pursued independently through the accepted methods of the sub-discipline; rather, it means that consistent, and vigorous attempts should be made to correlate, and reconcile the results of these independent lines of inquiry into a single, holistic, and congruous set of conclusions.

2.2 GOALS OF RESEARCH

Research into Proto-American prehistory is distinguished from the broader goals of anthropology by focusing on the Americas, with particular emphasis on reconstruction of the processes, and events which characterized the entry, spread, and establishment of the early populations and cultures of the Americas. Such a definition entails the discovery, and clarification of both (stochastic) historical realities, and broader processual (i.e., dynamic, process-oriented) issues.

If we accept the validity of both historical, and processual approaches to the study of Proto-American prehistory, then we can conclude that the overall approach to goal definition advocated herein can lead to a variety of interrelated, valid results. Not only can research lead to clarification of historical events and relationships, but these very events and relationships can be said to be, in part, the product of, and examples of, the operation of fundamental processual principles of broader interest and applicability to the rest of anthropology.

In defining the goals of research, other possibilities should also be discussed. Although one might hope for the resolution of the "impasse" which has dominated the field, such a resolution is a social process which can not be defined as a direct, and expectable outcome of scholarly research. There are two opposing points of view concerning the early prehistory of the Americas. However much one might agree or disagree with either point of view, it would be a mistake to assume that the goal of research should be aimed simply at demonstrating the ultimate validity of one of these two opposing points of view. The very existence of an "impasse" suggests that fundamental issues of much broader concern are involved in this "stalemate." If this work is to make a lasting contribution to the field, then the solution of the problems plaguing Proto-American prehistory should be a byproduct of the successful application of an alternative theoretical model which, in turn, should be seen as the principal contribution of this work.

2.3 EPISTEMOLOGICAL ISSUES

Nearly twenty years ago, Watson, LeBlanc, and Redman began their consideration of the process of "explanation in archeology" by stating that "the crucial issues are *epistemological*; that is, they have to do with how we know, and how we know we know" (Watson et al. 1971:3) (Emphasis in original). These questions are among the fundamental issues which must be addressed in this work. Yet the answers which we gain when we ask such questions are by no means the same for all those who ask. The work of a discipline is the collective investment of a community of researchers whose answers to basic epistemological questions, though sometimes shared, must always be reached one at a time—individually.

In an introductory paragraph devoted to a discussion of past styles of argumentation used in the "debate about early occupations in the Americas," Dincauze offers the following remarkable statement:

> With no established body of either theory or fact on which to base our judgments of evidence, and no sophistication in matters of epistemology, we are perilously exposed as we try to evaluate novel claims about 'truth' or 'reality.' Although it is possible to argue that logical positivism is flawed at its very roots—since both perception and explication are irremediably subjective—a commitment to its principles would be an improvement over the ad hoc eclecticism of current styles of archaeological thinking. The effort to establish canons of thought was one of the noble failures of the New Archaeology (Watson *et al*. 1971). Nothing has filled the void (Dincauze 1984:290) [Reference to "Watson, *et al*." in the original].

Perhaps our trust in the power of epistemological explanations is misplaced; or perhaps we have failed to grasp its unique dual role.

There is nothing inherently or ultimately compelling about particular epistemological approaches. Explanation addresses rational "canons of thought" within communally *shared frames of reference*. The explanation tendered by one is subject to evaluation, rejection, or acceptance by others. Acceptance or rejection of an explanation may be based on flawless logic; or on "common sense." Whatever the criterion employed, the adoption of an epistemological perspective is almost always an individual choice. And yet, its value is derived from the collective explanatory power it confers on those who share in it.

Epistemology ("how we know, and how we know we know"), is the essence of reason meeting the soul of conviction; here in both honesty and self-doubt, the confidence of a successful solution is forged. Any successful synthesis will be framed by its opponents and proponents. The necessary reduction of the complex world of experience to a set of internally consistent explanations which can be shared (or rejected) by others can always be challenged on epistemological grounds. However, legitimate challenges characteristically aim to further the broad ends of inquiry or the shared goals of the discipline. There is little virtue left in demonstrating that there are no "ultimate proofs" in scientific inquiry.

In the context of the theoretical model proposed below, the critical epistemological difference lies in the use of independent, yet mutually related lines of inquiry. Archaeology, and the data from prehistory are not abandoned. The model proposed in this work is not intended as a substitute for the data, and evidence provided by archaeology. Instead, external sources of evidence are used to clarify, test, and reinforce archaeological interpretations. Additionally, in the case of Proto-American prehistory, the use of independent yet mutually related lines of inquiry seemed particularly appropriate given the archaeological impasse which has dominated the field.

2.4 THE MODEL

The model presented in this work contains both elements of the perspectives with which we approach the definition of a problem (that which Kuhn 1970 calls a "paradigm"), and definitions of various methodological approaches associated with the model. The theoretical perspectives, and methodological components of the model are not necessarily new to the field; only the particular approach which joins them together, or their application to a particular class of phenomena may be new. Keeping these observations in mind, I would like to introduce the model employed in this work by identifying the principal theoretical and methodological sources from which I have derived the components of the model.

In the last few decades various segments of American anthropology have undergone often very painful self-analysis in an effort to redefine themselves and their methods under the labels of "science" or "natural science" (cf. Willey and Sabloff 1980:185-196; Spaulding 1968; Watson and Watson 1969; Watson et al. 1971; MacNeish 1978:60; Binford 1983:21; and Dincauze 1984:290). Often these discussions have centered around complicated, and esoteric philosophical perspectives. I do not propose to engage in such discussions at this point. Instead, I would like to suggest that the truest definition of a science is the product of its methods, rather than its philosophy; and that science can best be defined by "doing science" rather than by "discussing science." Stated in another way, I am explicitly adopting a perspective which holds that "science" or "natural science" is only worth pursuing in as much as it yields solutions to critical research problems.

James Lett defines science as follows:

> ...Science is a systematic method of inquiry based upon empirical observation that seeks to provide coherent, reliable, and testable explanations of empirical phenomena and that rejects all accounts, descriptions, and analyses that are either not falsifiable or that have been decisively falsified (Lett 1987:26).

To achieve these goals, Platt suggests that we should begin by:

> 1) Devising alternative hypotheses;
> 2) Devising a crucial experiment (or several of them), with alternative possible outcomes, each of which will, as nearly as possible, exclude one or more of the hypotheses;
> 3) Carrying out the experiment so as to get a clean result;
> 1') Recycling the procedure, making subhypotheses or sequential hypotheses to refine the possibilities that remain; and so on (Platt 1964:347).

These steps, in turn, will direct our choices away from alternatives which have been falsified through empirical tests—a process which Platt likens to following the branches or branching divisions of a "logical tree" (Ibid.).

Clearly the method of inquiry described above requires the generation, and testing of multiple alternative hypotheses. Without these, no choices can be made. However, Platt points out one additional, and highly significant reason for using alternative hypotheses.

> The conflict and exclusion of alternatives that is necessary to sharp inductive inference has been all too often a conflict between men, each with his single Ruling Theory. But whenever each man begins to have multiple working hypotheses, it becomes purely a conflict between ideas (Platt 1964:350).

"Ruling Theories" become too closely identified with individuals. A choice for or against a particular "Ruling Theory" falsifies nothing—nothing is resolved. At the same time, such a choice, unfortunately, amounts to or is perceived as no more than an *ad hominem* attack. Under these circumstances the focus of effort shifts away from the legitimate concerns of the discipline to a shallow, and irrelevant contest of personalities. What is needed, instead, is the necessary resolve to return the focus of effort to a resolution of conflicting ideas.

Finally, Platt offers one more note of caution. He says:

> Beware of the man of one method or one instrument, either experimental or theoretical. He tends to become method-oriented rather than problem-oriented. The method-oriented man is shackled; the problem-oriented man is at least reaching freely toward what is most important. Strong inference [the method of multiple hypothesis testing advocated by Platt] redirects a man to problem-orientation, but it requires him to be willing repeatedly to put aside his last methods and teach himself new ones (Platt 1964:351).

Thus it is my conclusion that the best methodological blueprint which might ultimately lead to a resolution of the problems which have plagued research into Proto-American prehistory lies in adopting the approach advocated by Platt, including a strong problem orientation, first, and foremost. I believe that the specifics of the methods employed in generating, and testing alternative hypotheses should originate from standard methodological techniques of the discipline, as suggested by the specific circumstances, and the nature of the hypotheses which are to be tested. This is the approach which I propose to use in this work.

Using currently available data, the model employed in this work attempts to explicitly frame a model output which can serve as a counterpart to the "Clovis first" culture-historic construct. Neither the model output proposed in this work, nor the "Clovis first" construct should be viewed as simple hypotheses, though each construct is capable of yielding a hierarchy of dependent multiple hypotheses amenable to direct testing.

By definition, a model is selective in its use of the data, and assumptions which it uses to create a model output. Thus, the specific model output may always be vulnerable based on these choices, and on the reliability of the data base used to construct the model output. On the other hand, the advantages of adopting a model, and a model output stem from explicit formulation, and their ability to give rise to falsifiable alternatives (i.e., Platt's preferred method of "strong inference").

More fundamental to the model proposed in this work is the expansion our perceived boundaries of the problem to include various lines of evidence outside the narrowly exclusive approach of relying only on the archaeological data of the Americas. This expansion of boundaries represents an approach which requires a redefinition of the scope, and goals of research in a manner that might be viewed as paradigmatic. This approach also entails some methodological consequences. When combined with the method of "strong inference," these methods require that:

1) one should not test a model output using the same data base from which the model output was generated; that

2) the model outputs must be aimed at finite, and concrete phenomena, operating through explicit rules within specific spatial, and temporal domains; and that

3) the model outputs must have specific consequences (typically expressed as hypotheses) capable of falsification by comparison to empirical bodies of data. As with other aspects of the model proposed in this work, this alternative approach to problem definition, and its methodological consequences can be traced to the approach employed by various anthropologists during this century.

Early in this century, as American anthropologists were attempting to come to grips with the chronological dimension of aboriginal cultures, Edward Sapir published a short monograph titled: Time Perspective in Aboriginal American Culture: a Study in Method (Sapir 1916). Sapir was writing in the early years of the discipline, when, in retrospect, holism was more a reality born of conviction and necessity than a tradition or the time-honored hallmark by which we characterize the field today. The details, and specific suggestions offered by Sapir may long since have been abandoned as a result of specific gains in data, and technique during the intervening decades, but the essence of his approach is still valid today.

Sapir's method was predicated on the realization that the cultural, and biological history of aboriginal America was a single (if highly varied and complex] phenomenon. Furthermore, Sapir recognized the scanty detail presented by the whole of American prehistory of his day, and the need to develop a methodology which might successfully access this information more completely. Therefore, Sapir advocated the interdependent use of multiple, independent lines of inquiry; including non-archaeological sources of evidence.

In the context of the present work, Sapir's suggestions form a starting point for problem formulation. As I have indicated earlier in this chapter, the scope of the problem is much broader than the way it is normally defined. In other words, the study of Proto-American prehistory requires a holistic perspective which recognizes that the initial peopling, spread, and ultimate biological, cultural, and linguistic diversification of the descendants of initial New World populations is a *single reality* which can not, and should not be defined either as an exclusively archaeological problem nor as a set of unrelated topics within the separate domains of the various subdivisions of anthropology except for the sake of the independent methods which have been developed within each subfield of anthropology.

Sapir's method was not intended to support attempts at "propping-up" sagging arguments, or to supply "missing evidence" in one sub-field by "borrowing" from other sub-fields. Rather, one might argue that all components of a "single phenomenon" should ultimately be able to arrive *independently* at comparable, congruous, or at least, non-conflicting solutions, even when the data bases and methods employed are not directly comparable. Conflicts between the independent results of two or more approaches must ultimately be resolved. Thus, an approach to problem solution which employs several independent methods, lines of reasoning, and lines of evidence can yield a set of limits or *disproofs* which can be used systematically to narrow the range of possible solutions, ultimately yielding effective, testable solutions.

Two other components of the model proposed in this work concern 1] the long-standing anthropological use of biocultural perspectives to the solution of complex research problems; and 2] the heuristic use of analogy in modeling past cultural dynamics.

Traditionally, anthropologists have subdivided the interests of the discipline into the biological, and cultural aspects of human existence. The theoretical model employed in this work reaffirms the close relationships, and interdependence between these realms. As with the evolution of non-human organisms, human evolution, and the evolution of individual cultures can each be viewed as highly complex non-linear historic events. By this I mean that while evolutionary changes are explainable by means of valid demonstrable processes and principles, these same principles, by themselves, are insufficient to permit accurate *prediction* of specific events over extended periods of time.

In many respects, transgenerational cultural stability and change behave very similarly to the highly complex processes, and events which characterize stability or change in biological populations. Cyclical systemic shifts are superimposed upon dynamic exchanges between niche realities, on the one hand; and internal shifts in proportions and the structuring of components, on the other hand, resulting in systemic stability or systemic change. In all this complex activity, nowhere is there a single linear sequence which overrides all other processes. In other words, the outcome is the combined interactive result of a wide range of principles, and components. This is the essence of non-linearity.

As a result of the non-linear nature of culture, we can make two additional observations. First, from a practical point of view, it is impossible to account for all the interactive components, and processes (initial or subsequent) which ultimately contribute to an outcome. Second, slight variations in the conditions of a complex non-linear phenomenon can lead to two or more equally possible outcomes, which in practice can not be operationally narrowed down to a single set of specific predictions or a single outcome. In other words, history, whether the history of the development of a species or of a culture, can have many equally valid potential trajectories which successfully defy specific predictions.

Given the above-outlined perspective, we may fairly ask what valid role or roles can the researcher play when dealing with non-linear historic realities?

Robert Dunnell has indicated one set of solutions based on a subdivision of the sciences into those fields which view reality from a "space-like" frame as opposed to those fields which view reality from a "time-like" frame (Dunnell 1982:8). According to Dunnell, these two approaches result in fundamental contrasts which "...are commonly expressed in terms of tertiary effects: 'how' versus 'why' questions; functional versus evolutionary or historical explanations; and proximate versus ultimate causation" (loc. cit.). Dunnell goes on to say:

> Just as important as the recognition that there are two kinds of science at the most fundamental level is acknowledgment that neither is right or wrong in absolute terms. Two distinct sciences may be compounded about the same subject phenomena (e.g., ecology and evolutionary biology). While such sciences interact through specific concepts common to both frames—'adaptation' for example—interaction should not blind us to their fundamental differences. Such dyads seek different kinds of explanations for phenomena conceived in different fashions (Dunnell 1982:10-12).

The relationships between such related or interacting sciences tend to be mutually beneficial. "The space-like sciences have the potential for contributing much about mechanisms..., while the time-like sciences can identify significance, a problem often quite intractable with space-like approaches" (Dunnell 1982:12).

As with the example furnished by Dunnell, the key to a mutually beneficial, and successful collaboration between space-like frame, and time-like frame sciences seems to lie in their shared conjunctions of processual conceptualizations and terminology. As Dunnell aptly points out, "adaptation," and other such shared processual conceptualizations allow the successful reconciliation of both synchronic, and diachronic issues. Unfortunately, so far, archaeology has been unable to establish such a successful collaboration with a space-like frame science.

In an earlier article reviewing the incorporation, and use of evolutionary theory in archaeology and ethnology, Dunnell notes that "...if evolution is taken to mean what it does in the sciences, it has yet to be systematically applied in either sociocultural anthropology or archaeology" (Dunnell 1980:37). Dunnell goes on to point out that

> Archaeologists have had no illusions about contributing to evolutionary theory. They have been consumers from the very beginning, and they have relied heavily upon theoretical developments in sociocultural anthropology and biology (loc. cit.).

Undoubtedly some archaeologists in the new archaeology saw their role principally as consumers. Spaulding, for example, notes that "...archeologists must be regarded as primarily consumers rather than producers of anthropological knowledge and theories" (Spaulding 1968:37). Although the title "Archaeology as Anthropology" might seem to suggest a similar sentiment, it is significant that Binford began by viewing archaeology's role, not as a consumer, but as a fully contributing partner. Binford notes:

> Archaeology must accept a greater responsibility in the furtherance of the aims of anthropology. Until the tremendous quantities of data which the archaeologist controls are used in the solution of problems dealing with cultural evolution or systemic change, we are not only failing to contribute to the furtherance of the aims of anthropology but retarding the accomplishment of these aims (Binford 1962:224).

This quote, perhaps more than any other, points to the initial perception of archaeology's aspirations at establishing a collaborating (time-like frame) partnership with a (space-like frame) sociocultural anthropology or ethnology endowed with an evolutionary, processual orientation. Unfortunately, these early aims were never met—while efforts by biologists to provide the space-like frame lacking in ethnological theory have met with overwhelming (though understandable) resistance.

In the past, American archaeologists have relied heavily upon the use of analogy to provide the necessary conjunction between archaeological data, and synchronic interpretations (such as those used by sociocultural anthropologists). In a more formalized sense, it is possible to divide the nature of analogs and their uses into two varieties—namely, "ethnographic analogy" and "general comparative analogy" (Willey and Sabloff 1980:205-206). Watson, et. al. (1971:50) offer a similar dichotomy in the use of analogy between the "...direct historical approach and the general comparative approach."

Sharer and Ashmore (1979:462) state that the direct historical approach is "...a method of reconstructing prehistoric societies by progressive extensions of analogies back through time." They go on to point out that "to be successful, an analogy should involve a society that possesses the same degree of overall complexity as that indicated for the prehistoric situation" (Sharer and Ashmore 1979:464). Furthermore, successful analogies are also dependent upon using analogs drawn from comparable environmental contexts in the not too distant past. Indeed, Sharer and Ashmore point out that "...as the time span between the analog and the archaeological data increases, the chances for other variables to distort the reconstruction also increase" (loc. cit.). Thus, we must reject the application of the direct historical approach to the modeling of the cultural dynamics of Pleistocene societies.

In a separate discussion, Richard Gould notes that:

> Uniformitarian laws in the sense used by natural and physical scientists explain the patterned behavior of natural phenomena in the aggregate, but... such laws do little to account for unique events that tend to occur in the historic and prehistoric record of human behavior. The archaeologist, unless he arbitrarily chooses to limit himself, is concerned with explaining both regularities and unique occurrences in past human behavior, and the inferences archaeologists use to make their data 'speak' should be appropriate to these dual concerns (Gould 1978:250).

Gould's point is well taken. A clear distinction must be drawn between the "unique occurrences" or events which characterize the history of human behavior; and the aggregate, patterned behaviors illustrating the underlying principles which govern all natural phenomena, including human behavior. The direct historical approach is a powerful technique which allows us to access past unique historical behaviors in a manner which insures accuracy through replicable testing, and corroboration. Unfortunately, given its inherent requirements, the direct historical approach is severely limited to the proximal end of the temporal scale. If one wishes to understand temporally more distant portions of the archaeological record, other approaches must be employed.

The alternative might appear to be the selective adoption, and selective rejection of specific natural laws. Some obviously apply to humans, while others are rejected or restricted to insure a set of outcomes which agree with preconceived expectations. Unfortunately, in the natural sciences, the restriction or rejection of applicability of natural laws must be explained; and those explanations must be tested, and empirically demonstrated before they can be accepted. To reject or restrict operating principles or natural laws simply because "humans are different" amounts to special pleading. In the view of at least one researcher,

> The human being is a sufficiently special kind of animal and sufficiently dear to all of us to assure the social sciences a permanent place in our scientific establishment. But if the social sciences are to progress they must demystify human behavior. There is evidence that, because of the

> central role of deceit in the conduct of human affairs, we are singularly resistant to theories that demystify human behavior. Such may be the main basis of opposition to reintegrating the social sciences into a theoretical framework that accounts for the evolution of all life forms on this planet (van den Berghe 1982:27).

What we can, and should acknowledge is that human behavior, and especially grouped human behavior, is a highly complex, non-linear phenomenon which can not be successfully or completely explained with simpler linear statements of cause-and-effect. In addition to the use of a non-linear systemic approach, we must assume that all other natural laws apply to humans and human behavior, unless we can clearly explain, and empirically demonstrate why and under which conditions they fail to apply.

In order to avoid the problems associated with strictly linear explanations, we must integrate into our modeling three key ingredients. First, we must seek to identify the underlying principles which govern ethnographic examples, and universal aspects of human behavior or culture, while seeking to identify the component principles which they share with other, known, principles from the natural sciences. Second, we must extend our capacity for drawing analogues to non-human, "natural" examples, seeking all along to identify, and test the possibility of extending the natural principles which govern non-human examples to infer or explain "human phenomena." These two initial ingredients represent two corresponding aspects of a process which seeks to reconcile observations from the natural, and social sciences. Third, we must avoid adopting an extreme reductionist point of view by adopting a non-linear or systemic perspective. While many law-like relationships in the natural sciences can be reduced to a set of statements expressing cause-and-effect linear relationships (i.e., within space-like frames), many others can not be reduced to this level without confusing or trivializing the process under consideration. It would certainly be naive, and a sad mistake to limit our conception of the natural sciences as operating strictly on the basis of simple linear cause-and-effect laws.

By incorporating an awareness of the importance of (time-like frame) non-linear concerns, one is more likely to avoid the errors associated with the perceived necessity to develop at any cost, often trivial linear cause-and-effect "laws" of human behavior which Flannery (1973:51) characterized as "Mickey Mouse laws." As Flannery notes,

> These laws have even emerged from the lips of colleagues whom I regard as sane, serious, and competent men. For example, at a genuinely exciting seminar on the Bushmen I learned that 'the size of a Bushman site is directly proportional to the number of houses on it.' From a Southwestern colleague, I learned last year that 'as the population of a site increases, the number of storage pits will go up.' I am afraid that these 'laws' will always elicit from me the response, 'Leapin' lizards, Mr. Science!' (loc. cit.).

I firmly believe that it is possible to develop broad explanatory principles of human behavior—something along the lines of what Harris (1968:1-3) might call "nomothetic laws" of human behavior. A logical starting point for deriving such explanatory principles might be at some broad universal generalizations about human behavior and culture, as obtained from careful cross-cultural comparisons. It must be specified, however, that the formulation of "nomothetic laws" of human behavior lies outside the scope of this work. At best, this work seeks to explicitly draw general ethnographic analogies, comparing them, and integrating them with various principles derived from the biological sciences. The focus of these efforts in the subsequent chapters remains squarely with various direct attempts at establishing boundaries, parameters, and generally modeling those aspects of the cultural dynamics of Pleistocene Native American cultures which might be reflected in the prehistoric record. In contrast, the preceding detailed discussions are offered only as a means to explicitly establish the theoretical orientation of the model employed in this work.

2.5 MODELING, AND THE GENERATION OF THE MODEL OUTPUT

As I have indicated earlier, the model employed in this work rests squarely upon anthropology's long-standing acceptance of a biocultural perspective to the study of human phenomena. This perspective is responsible for the incorporation of the methods and theory of the biological sciences (i.e., within physical anthropology) in a discipline which is equally concerned with the nature, and operation of culture. This theoretical perspective is not only allied to the methodology proposed by Sapir (1916), but it is also responsible for the theoretical underpinnings of modern paleoanthropology (cf. Wolpoff 1980:3; Weiss and Mann 1990:11; and Straus 1991:125, 126-127).

The process of modeling should be governed by explicit guidelines. Three general guidelines of modeling were introduced earlier, following the discussion of the method of "strong inference" presented at the beginning of the last section (section 2.4). Given the subsequent discussions about the importance of the biocultural perspective, and the use of analogy relative to the problem of Proto-American prehistory, we may go on to propose the following problem-oriented guidelines:

1) the events which characterized Proto-American prehistory should be viewed as a single set of realities explainable from a biocultural perspective which integrates cultural data with relevant data from physical anthropology, and the natural sciences;

2) the model output of Proto-American prehistory should be generated independently from the archaeological data recovered from early New World sites;

3) the model output of Proto-American prehistory should address a broad range of issues from a diachronic perspective. These issues should include considerations of the source, movement, population numbers, and cultural dynamics of the initial immigrants, and their descendants. Given the limitations imposed by the preceding guideline, the relevant model outputs should be broad based, and comprehen-

sive; and they should be derived from a consideration of various sources including elements of general comparative analogy drawn from small-scale societies, human biology, demography, paleoanthropology, Old World prehistory, and paleoenvironmental studies; and

4) the holistic model output of Proto-American prehistory should be capable of generating relevant hypotheses amenable to subsequent testing through the totality of the empirical data obtained from Proto-American archaeological sites in the Americas. The remainder of this work is dedicated to the implementation of these propositions.

Insofar as the model output addresses a range of issues which are to be tested, and compared to archaeological data, a set of relevant factors are reviewed from a mutually interactive perspective in chapter three. These factors include the cultural dynamics of subsistence, division of labor, resource allocation, ecological factors, reproductive and demographic alternatives, enculturation, group cohesion and fissioning, and culture change within the context of small-scale societies. The data, and the principles used in modeling the dynamics of these factors, are derived from the published literature in anthropology, biology, and demography. In these discussions it is assumed that the first immigrants, and their descendants, were hunter-gatherers forming small-scale societies. Based on the principle of uniformitarianism, general ethnographic analogy is coupled with the use of ecological, demographic, and general biological principles in modeling the cultural dynamics of these early populations. Archaeological data, and particularly data from the early prehistory of the Americas is excluded from these discussions.

The subsequent chapter is aimed at establishing the specific temporal, and spatial context of Proto-American prehistory. Once more, data derived from the early prehistory of the Americas is excluded from these discussions. Instead, this chapter draws on the data of physical anthropology, genetics, demography, paleogeography, paleoecology, paleoclimatology, and the Pleistocene prehistoric record of the western rim of the Pacific. The chapter aims to establish an independent set of limitations within time, space, which can then be used as the basis for specific discussions of relevant paleoenvironmental parameters, and Old World cultural antecedents.

Chapter five draws upon the discussions and projections presented in the preceding two chapters to generate a broad culture-historic construct—the model output. The subsequent chapter, in turn, draws upon the model output to generate a set of hypotheses, and specific predictions relative to the early prehistoric (archaeological) record of the Americas.

Following an initial discussion of the criteria through which archaeological data should be evaluated, chapter seven presents a limited survey of sites of Pleistocene age reported in the literature. This chapter is not intended as a formal test of the validity of the model output, but merely as a demonstration of the applicability of the predictions presented in the preceding chapters to the evaluation of the early archaeological record of the Americas. This demonstration is continued in greater depth of detail in chapter eight, where case studies are used to review the data yielded by two early sites. The more detailed discussions presented in this chapter are used, in turn, to illustrate some of the preferred standards of archaeological investigation which should guide future archaeological research into Proto-American prehistory. The overall process of modeling, demonstrated in chapters three through eight, represents an example of the application of the model which was introduced in this chapter. Hopefully the approach to problem resolution employed in this work will stimulate discussion, and broaden the range of options with which we approach the solution of this, and other future research problems.

Chapter Three

SIGNIFICANT FEATURES OF SMALL-SCALE SOCIETIES

The operation of cultural systems as presented in the remainder of this work is limited by the exclusion from consideration of any cultural systems which rely on food production for any of their subsistence. In other words, given the ultimate goal of presenting a theoretical approach capable of generating hypotheses related to the Pleistocene occupants of the New World, the introduction of additional variables seen in some of the larger and more complex cultural systems which have arisen during the Holocene would simply introduce additional irrelevant variables to subsequent discussions. Therefore, the scope of subsequent discussions has been limited to what have been sometimes referred to as "simple," "small-scale," "hunter-gatherers," or "band and tribal level" societies.

An important and expected outcome of excluding from consideration all but small-scale societies is the opportunity which these limits afford to define convenient units of cultural systems tied to specific human demes. These societies are separated and relatively isolated from other such units by geographical isolation, differential degrees of within-group to without-group interactions, and a high level of internally shared and integrated cognitive and behavioral traits. The definition of such units does not deny the existence or operation of larger, regional mating networks or interactive cultural aggregates, nor does it exclude smaller subsystems within these cultural systemic units.

As discussed earlier, the principal aim of this chapter is to formally introduce and discuss a range of topics and operant principles which will be used to construct the model output of Proto-American prehistory. These topics and principles concern the cultural dynamics of subsistence, division of labor, resource allocation, ecological factors, reproductive and demographic alternatives, enculturation, group cohesion and fissioning, and culture change within the context of small-scale societies. The data, and the principles used in modeling the dynamics of these factors, are derived from the published literature in anthropology, biology, and demography. The approach to modeling employed in this work assumes a uniformitarianist perspective. This approach relies on the use of general ethnographic analogy, and on various principles derived from ecology, demography, and evolutionary biology. It is further assumed that this approach makes use of the optimum source of data and principles necessary to accurately model some generalized aspects of the collective behavior and cultural dynamics of small-scale societies of hunter-gatherers.

Specific subsections of this chapter will focus on some key topics. These topics include discussions of:

1] the importance of biocultural perspectives of energy and subsistence to the understanding of small-scale societies;
2] "individual" versus "systemic" approaches to foraging;
3] questions related to the acquisition and use of environmental information, relative to foraging choices;
4] demographic factors, including growth rates;

5) the dynamics of population stasis and growth;
6) mechanisms influencing grouping and dispersal tendencies; and
7) the dynamics of cultural stability and culture change in small-scale societies.

Each of these topics should be viewed from the perspective of the ultimate goal of this section, that is, to provide an explicit basis by which the human biological, culture-historic, and biogeographic events of Proto-American prehistory can be modeled. The model output, presented in chapters five and six, draws directly on the data, processes, and principles discussed in this chapter.

3.1 SMALL-SCALE SOCIETIES: BIOCULTURAL MODELING OF ENERGY AND SUBSISTENCE

In order to model the cultural dynamics of Pleistocene-age societies, this section will attempt to integrate data from general ethnographic analogy with some fundamental principles developed in biological ecology. The significance of general ethnographic analogy lies in that it is not directly tied to stochastic or historic attributes of specific cultures. This permits us to model small-scale societies as functioning units, while avoiding reference to attributes unique to specific small-scale societies. This perspective also allows us to introduce into the modeling process a variety of operant principles from biological ecology and evolutionary biology which apply equally (without reference to particular species) to groups and individual organisms. One fundamental currency through which biological principles are explained is energy flow.

A prime component of the perspective which I propose to take to the study of small-scale societies begins with recognition that these cultural systems are valid integrated units in their own right which interact with ecosystemic components through multidimensional hypervolume relationships. Niche axes are multiple and interactive, each characterized by minimum and maximum levels which the system can tolerate. The underlying principles, which permit the application of Shelford's law of tolerance (see Odum 1971:107-108) to biological organisms (systems in their own right) in ecological contexts, also find equivalent (not analogous) expressions in the integration of cultural systems of small-scale societies within their larger ecosystemic contexts.

A small-scale society can be defined as a systemically integrated local unit composed of culturally and biologically interacting individuals. Just as the source, volume, and energy budgets of individual organisms and trophic classes yield significant insights about the dynamics of biologically defined ecosystems and their components (cf. Odum 1971; Rickleffs 1979; Smith 1986), so too these factors can be profitably analysed relative to the operation of another unit—small-scale societies (cf. Jochim 1983; Keene 1983; Smith 1983). Energy flow can be viewed as the common currency which drives both cultural and biological systems within small-scale societies. In the following discussion I propose a series of dynamic relationships relevant to small-scale societies which are modeled on the basis of biological and ecological principles and analogues.

In small-scale societies purely biological energy requirements must be prioritized and balanced relative to cultural energy allocations through integrated systemic decisions, rather than as distinct contrastive expenditures. A basic principle governing energy allocation choices in small-scale societies emanates from the relative nearness of human biological systems to the sources of energy used by small-scale societies. Since energy is channeled first through organismic (human) subsystems; and since cultural systems are wholly dependent upon these subsystems for their existence and continued operation, the bulk of the energy flow entering small-scale cultural systems always tends to be allocated preferentially to meet the various requirements of the component organismic subsystems.

The relative degree to which systemic requirements become prioritized (within limits) ahead of individual biotic requirements increases in direct proportion to the number of individuals which interact within a cultural system. In small-scale cultural systems, composed of few individuals, each individual exerts a more profound impact on setting systemic goals and in mediating between individual and systemic (cultural) requirements.

Egalitarian social organization and temporal variations in band composition (through fission-fusion) are two outcomes of the high impact of individuals on small-scale cultural systems. Stability of group membership, accompanied by increased efforts at group identity definition, group solidarity, larger and more complex social organization, increased specialization, social and information ranking and concentration (all requiring increased systemic energy investments) are directly dependent on increasing numbers of individuals in single cultural systems. The development of these characteristics, in turn, require increased steady or cyclic volumes of energy flow for their maintenance.

As homeothermic mammals, human beings require relatively regular inputs of food energy, nutrients, and water to meet metabolic requirements. These requirements may be met only cyclically from (variably) "patchy" natural environments.

The single largest and most efficient form of energy storage available to small-scale cultural systems is human biomass. Fat represents an especially concentrated and important component of energy stored in the form of biomass (cf. Speth 1983:145-159 for additional discussion of dietary and bodily fat among hunters). Body fat and excess number of individuals (beyond the minimum number of individuals needed to meet reproductive and culture-systemic needs) constitute efficient systemic forms of "storage" which allow the system to regain systemic homeostasis following periods of stress due to decreased energy flow. Biological capacities and tendencies to store body fat, and reproductively to exceed death rates (a basic premise of natural selection) therefore contribute significantly to culture systemic homeostasis.

The limiting factors incurred by these strategies, on the other hand, are twofold:

1) Increased body fat results in increased energy expenditures in task performance which yield decreased work efficiency. These de-

creases in efficiency render the capture and transformation of food energy into work more energy expensive per net unit of energy gained by the system.

2) Increased population densities can severely alter the niche relationships (i.e., the character, composition, and adaptive strategies) of a cultural system. The operation of these limiting factors in the context of small-scale societies points to inherent difficulties in the use of the concept of carrying capacities.

Cultures respond demographically and through changes in systemic organization and technology—all of which are dynamically interdependent within the overall cultural system. To exclude from consideration any of these components from the total equation will invariably result in errors in prediction or interpretation.

The cultural system's capacity to respond dynamically to various intrinsic and extrinsic factors results in what is commonly referred to as culture change. Cultural uniformity or homogeneity always has a relative, rather than an absolute value. Variety and variability are inherent properties of all cultures.

> Variety is continually generated in every human population. This variety exists in two forms. On the one hand, individuals know how to accomplish a given task in ways that are different from the ones they are actually practicing. On the other, there are individuals in the population who are accomplishing the end in different ways. That is to say, variety exists both in the minds of individuals who know of alternative behaviors but are not practicing them, and by the actual practice of such strategies by deviant individuals within the population. This variety has a number of potential sources, including contact with other populations, mislearning, invention, and boredom (Hill 1977:45).

Variety and variability should not be equated with culture change. Culture change results from dynamic systemic responses to extrinsic or intrinsic factors which make use of the resident alternatives provided by systemic variety and variability.

Culture change (as changes in technology and systemic organization) becomes a homeostatic (potentially adaptive) mechanism whose action performs the same (not analogous) function as changes in genetic composition of a naturally selected (non-culture bearing) population. Both culture change and changes in gene frequencies can be viewed as homeostatic responses to changes in the dynamic relationships between open systems and their environments. I hasten to point out, however, that this notion is not the same as equating culture change (a dynamic outcome), with natural selection (an interactive process).

Changes in the potentials and limitations caused by intrinsic and extrinsic components affect all the components and their mutual relationships. Ultimately, however, such changes must be described as they affect all the components of the equation, allowing an active role for each. These changes may be viewed (artificially) in temporal isolation for the sake of convenient dissection and description.

There are inherent problems in equating culture systemic attempts at homeostasis (an on-going property of self-perpetuating open systems) with "adaptive" responses. To begin with, what constitutes an "adaptive" response in one context may not be "adaptive" in other contexts. Furthermore, given the multiplicity of functions performed by cultural systems, no response can be viewed as wholly or purely "adaptive" from all perspectives. Cultural systems, just as organisms within a multidimensional hypervolume, must constantly make "tradeoffs" in their choices. Still another point can be made that "adaptation" is a relative state which changes through time. Thus, there is no ultimate adaptive condition; while a retrospective definition of what constitutes "adaptive" or "preadaptive" systemic responses amounts to a teleological argument.

In the context of cultural systems the use of the concept of "adaptive" responses is further clouded by reference to "rational" capacities of humans. Although I do not intend to challenge human "rationality," I believe that it is a grave mistake to equate a capacity for "rational" thinking or actions with an obligatory outcome of "adaptive" logic or behaviors. This issue is potentially even further clouded by the adoption of a perspective which assumes 1) a perfect or complete awareness of all intrinsic or extrinsic factors and their interrelationships, and 2) an unlimited capacity to produce the most "adaptive" systemic responses. "Real decisions are usually shortsighted approximations characterized by restricted knowledge, faulty perceptions, and limited calculating abilities" (Jochim 1983:159). Furthermore, the "availability" of environmental resources to human groups requires not only their presence (in relative abundance) in the group's territory, but also the means (often technological and systemic) to access specific resources. In many cases, lacking the technological or systemic means to access a potential resource effectively makes that resource unavailable for use. Some of the more pointed criticisms of the applicability of "optimum foraging theory" to human beings can be viewed as expressions of objections to these and related problematic assumptions (cf. Jochim 1983; Moore 1983).

A distinction should be made at this point between these criticisms of optimization models and the inherent validity of their predictions. There is a great difference between criticisms which question the fundamental mechanisms advocated by such models, as opposed to criticisms about the manner of their application to predicting human behavior. Given the similarities between the approach of optimal foraging theory and many of the components of the theoretical approach which I advocate herein, I shall attempt to discuss briefly some of these correspondences.

Speaking about the anthropological application of optimization models developed in fields such as economics and ecology, Jochim (1983:157) notes that optimization models have met with "...growing dissatisfaction with their use in these other fields." Yet later in the same article he concludes that "given the problems of all models, including optimization models, it is not being recommended [by Jochim] that they be abandoned" (1983:167). In a separate critique of optimal foraging theory, Keene (1983:148) concludes that "optimal foraging theory is a valid mode of inquiry. It is also a limited mode of inquiry, and I do feel it is important to recognize its limitations as we should be aware of the limitations of every method or theory we use." I heartily concur with this last conclusion.

In a recent overview of optimal foraging theory and its application under a variety of controlled and experimental conditions in biological ecology, Robert Smith (1986) gives no indications that optimal foraging theory is being rejected or abandoned by biologists. A similar view is echoed by Graham Pyke, who in an extensive review of optimal foraging theory in biology, concludes that despite some specific shortcomings, "the optimal foraging approach seems worth pursuing" (Pyke 1984:566).

> Observation and experimental studies support the hypothesis of optimal foraging up to a point. It is not surprising that they should. Much optimal foraging theory concerns actions you would expect any mobile animal to take: forage in areas where food is abundant, leave when searching is no longer rewarding, select the most palatable and larger items of food, leave the poor items until last, and don't travel any farther than necessary to feed. Where the theory breaks down is in the expectation that the animals will select patches in order of their profits or take only optimal food items first and ignore the rest. Such choices may be characteristic of animals foraging in a stable laboratory environment. It is not necessarily the way animals behave in the wild.
>
> Being opportunists, animals will take some less than optimal food items (by ecologists' standards) upon discovery, and they may quit before food items are reduced to some minimal level. Nor will they pass up certain profitable patches because they do not meet some theoretical expectation. Animals quickly learn where food is and where food is not, and they don't waste much time on a patch after it is depleted. Foragers, however, will stay with a patch as long as the rate of replenishment exceeds the rate of depletion (Smith 1986:317).

As with the unyielding application of any single theoretical perspective, the application of optimal foraging theory to biological or cultural contexts exacts a price by failing to consider the whole of the problem broadly enough to incorporate other significant intervening variables. Foraging strategies are simply one component of the overall dynamic integration between organisms (or cultural systems) and their respective ecosystems. Discussing the application of optimal foraging theory in ecology, Robert Smith concludes that,

> In spite of its weaknesses, the concept of optimal foraging is valuable. It provides a basis or scale against which to measure or compare how animals do forage. By doing so, the theory provides a better insight into how animals select and harvest their food (Smith 1986:317).

A similar conclusion is reached independently by Arthur Keene concerning the application of optimal foraging theory to the study of past cultures. As Keene puts it,

> It can be argued that the value of optimization models is not necessarily to prove that organisms or groups optimized... rather, the real value comes when optimization is used heuristically as a theoretical base line against which we can compare observed behavior. The most interesting results may actually be attained in measuring deviation from the theoretical optimum (Keene 1983:146).

These deviations from predicted values may be due to imperfect knowledge of the environment and its resources (overall, or at any given time); or they may represent compromises between foraging efficiency and other biotic requirements addressing other niche axes (from availability of drinking water, or plant nutrients, to social or reproductive requirements); they may be due to errors in our own perceptions and definitions of the specific components of a particular test case; or they may even be due to stochastic, individualistic factors in the environment or within forager groups which may possibly never be amenable to ultimate explicit reduction into a set of logical propositions capable of generating valid predictions. Each of these can be viewed as intervening variables which can ultimately modify the expression of the "base line" principles expressed in optimal foraging theory, but which, in themselves, do not invalidate the inherent principles expressed in the theory.

I am in basic agreement with Keene's statement that the observed departures from optimal predictions can be the most interesting and heuristic by-products generated by the application of optimization models to specific test cases. Still, if this argument were to be used primarily as a means to justify an approach which ignores or rejects the tremendous predictive power of optimization models in favor of focusing solely on the deviations from its predictions, I would have to judge such an approach as one-sided and grossly inappropriate. I do not believe that Smith (1986), Keene (1983), or others are deliberately proposing that we should adopt such a position. Instead, their comments serve to highlight the overwhelming heuristic value of a theoretical approach which yields valuable insights even at times when its specific predictions are not borne out by the data.

Although optimal foraging theory in anthropology is based on biological optimal foraging theory, which in turn is tied to several other contributing fields (most notably, economics), my primary concern here is its applicability to anthropological contexts. The literature on this topic is vast and growing, precluding any attempt on my part to provide a comprehensive summary. Instead, I have chosen to refer to a few key components, drawn from several sources (Bettinger 1980, 1987; Pyke 1984; and Smith 1983), which will provide a suitable starting point for further discussion. Subsequent discussions of operant principles derived from optimal foraging theory will be incorporated into the generation of the model output presented in chapter five of this work.

Fundamental to optimal foraging theory is the proposition that an organism's fitness (as defined in biology or physical anthropology) depends in part on its approach and success while foraging (Pyke 1984:523; Smith 1983:626).

> Most foraging models assume that foragers will be selected to behave so as to maximize the net rate of return (of energy or nutrients) per unit foraging time. This assumption seems reasonable under a variety of conditions, including the following: (1) available food energy is in short supply (fitness is energy-limited); (2) specific nutrients are in short supply (fitness is nutrient-limited); (3) time for adaptive nonforaging activities is scarce (fitness is limited by time available for nonforaging activities); or (4) foraging necessarily exposes the forager to greater risks (fitness costs due to predation, accident, climatic stress, etc.) than do nonforaging activities (Smith 1983:626) [emphasis in original].

Darwinian fitness involves a "sorting" process wherein individual differences in fitness, in the context of specific environmental parameters, express themselves through differential reproduction over a succession of generations. Organismic "strategies," whether real, imagined, or metaphorical, are not part of Darwinian fitness. Thus if we are to avoid violating the link between optimal foraging behaviors and Darwinian fitness, we must stipulate at the onset that terms such as foraging "strategies" or foraging "decisions" are simply metaphorical expressions for alternate foraging behavioral outcomes.

From this perspective, an organism's fitness will ultimately be expressed by the relative proportion of its traits which make up the pool of traits in successive generations. "This contribution may be measured genetically or culturally as the proportion of an individual's genes or 'ideas,' respectively, in the next generation" (Pyke 1984:523) [emphasis added].

Clearly, systematic studies of differential fitness require that optimal foraging behavioral traits should be capable of transmission transgenerationally. This statement, however, should not be construed to mean that there is a mandatory genetic component to the transmission of certain behaviors. As Smith notes, "...there is no reason to believe that any species inherits alternative foraging strategies via direct genetic transmission" (Smith 1983:626). More specifically, Pyke notes,

> ...it is assumed that there should be a heritable component of foraging behavior, i.e., an animal that forages in a particular manner should be likely to have offspring that tend to forage in the same manner. This heritable component can be either the actual foraging responses made by an animal or the rules by which an animal learns to make such responses. In other words, optimal foraging theory may apply regardless of whether the foraging behavior is learned or innate (Pyke 1984:523).

As Smith (1983:626) points out, such an approach to the transgenerational transmission of optimal foraging behaviors is "...decidedly not a form of genetic determinism."

Anthropological treatments of optimal foraging theory frequently view energy as a convenient, measurable, "proxy currency" to be used for analytical purposes (cf. Smith 1983:626; Keene 1983:143; Jochim 1983:159). I am in fundamental disagreement with an approach which categorizes energy merely as some type of conceptual "proxy currency." Energy and energy flow rates are empirical realities from a physical and biological point of view. Organisms exist as viable systems by capturing, channeling, and temporarily storing (potential) energy as biomass. Thus energy should be viewed not as some convenient "proxy" measure; but rather we should acknowledge its fundamental role in the composition and operation of all life. Therefore the net rate of energy return, so fundamental to optimal foraging theory, should be viewed as another expression of a common ecological measure known as "net productivity." This shift in perspectives is much more fundamental than simply a semantic difference in terms.

The perspective which equates net rate of energy return with net productivity focuses on the whole organism and the totality of its biological interactions. An organism's fitness depends not only on its foraging energy "gains" and "costs," but on how these gains and costs are balanced within the context of its total round of activities. Foraging behavior resulting in energy gains must be balanced not only against direct foraging costs, but against the energy costs by which foraging behavioral variants influence the organism's capacity to meet other biological requirements. Once these other costs are factored in, the net rate of energy return becomes identical with net productivity.

This perspective is particularly germane to an anthropological application of optimal foraging theory to the study of small-scale societies because it extends the costs associated with foraging strategies to include the production and maintenance of the local cultural system. Since Homo sapiens by definition must rely on some form of cultural system for his continued survival, the production, maintenance, or participation of individuals in cultural systems becomes a valid biological cost which must be factored into any accounting of net rates of energy return from alternative foraging behaviors.

It must be remembered that despite the intimate dynamic relationship which exists between an organism's foraging behavior and the remainder of the behaviors exhibited in the total round of living of that organism, optimal foraging theory is designed to focus exclusively on alternative foraging behaviors. Optimal foraging theory was not designed to address how organisms meet other (and from the point of view of fitness) equally important biological requirements. Failure to incorporate this realization into the analysis of organismic behaviors which focuses on foraging strategies will produce paradoxes in observed behavior which can not be ultimately reconciled exclusively from the point of view of optimal foraging theory.

The topics addressed by optimal foraging theory can be broadly subdivided into two interdependent categories. Optimal diet (diet breadth) addresses issues related to net energy returns resulting from alternative prey choices which bring different quantities of energy and different nutrients gained per prey captured, and different expenditures of time and energy in capturing and "handling" or extracting energy and nutrient benefits from each.

The broader category of optimal foraging efficiency addresses the net energy returns resulting from variable behaviors classified under four specific categories: 1) environmental patch choice; 2) foraging time allocation (per patch); 3) dispersal and group movements (including foraging group size); and 4) central place foraging or settlement patterns relative to foraging patches (cf. Smith 1983; Pyke 1984; Bettinger 1987).

The approach which equates allocations of time to various activities with allocations of energy to those activities, clarifies the emphasis on search time, and pursuit and handling time, as used in optimal foraging theory. Separate time costs associated with search time and handling time per numbers of species in the diet can be balanced against energy gains per prey unit harvested. The theoretically optimum number and mix of species in the diet can be defined as the combination of numbers and specific prey species which give the greatest combined net energy returns relative to both search time and handling time costs.

> To simplify the model, we shall consider only the case in which prey species have identical nutritive value but vary in abundance and predator escape tactics. The addition of a new kind of prey to the diet has two opposing effects on the rate of consumption of prey: first, a broader diet makes more individual prey potentially available to the predator; second, the average ease of pursuit, capture, and consumption decreases provided, of course, that the predator always chooses the prey species in descending order of suitability. A predator should broaden its diet until the decrease in the average quality of its prey more than offsets any decreases in searching time due to increased abundance of prey (Ricklefs 1979:756).

Figure 3.1 (below) offers a graphic representation of the relationship between time (energy) costs devoted to searching the environment for prey (Ts), and pursuit and handling time (Tp). The vertical axis shows time costs incurred per prey unit. The horizontal axis shows a sequence of prey types ranked in descending order (from highest ranked at left) according to the net energy return per prey unit handling time (based on Ricklefs 1979:756; and Smith 1983:628).

Search time (Ts) is most energy costly to the organism when focused on a single prey type (in this case, the highest ranked prey type at the left of the graph). As additional prey types are added to the total, encounters with prey become more frequent, and energy costs expressed as search time (Ts) decrease.

Because prey types are ranked according to net energy returns (with highest net energy returns at the left side of the horizontal axis), as an organism begins to broaden its niche width by adding more (and therefore lower-ranked) prey types, its net energy returns will decrease (i.e., pursuit and handling time relative to returns will *increase*). This factor is represented by the Tp curve on the graph in Figure 3.1. "The sum of the two (Ts + Tp) is inversely related to rate of prey consumption. Hence optimum diet breadth corresponds to the minimum of the (Ts + Tp) curve" (Ricklefs 1979:756).

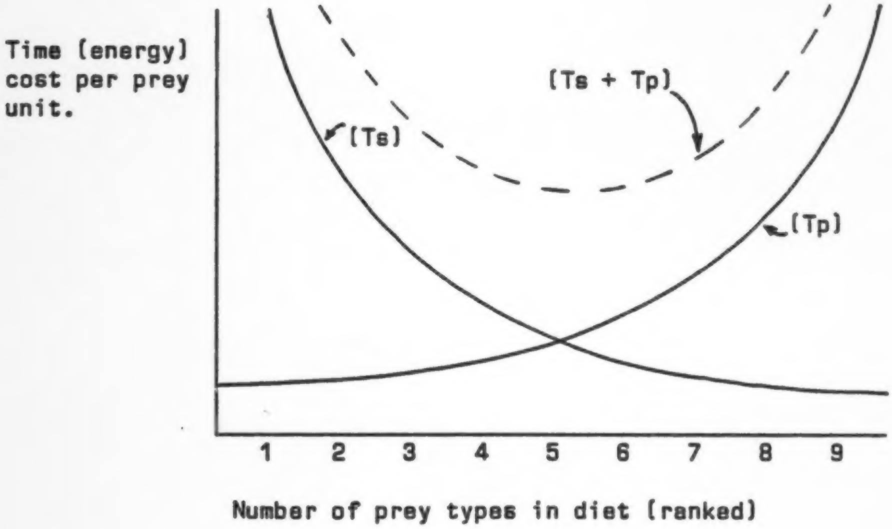

Figure 3.1
Diet-Breadth Optimization Interactions

Natural environments can be classified as fine-grained or patchy, depending upon the relative degree of (homogeneous) resource dispersal as opposed to (heterogeneous) resource clumping within a given area. The graph presented above was designed to predict diet breadth optimization under fine-grained environmental conditions. Additional variables, such as travel time between patches, differences in patch productivity and types of prey between patches, and patch resource depletion must be incorporated into the fine-grained environmental model output in order to apply the model's predictions to patchy environments.

When applied to fine-grained environments, the optimum diet-breadth model output requires that we adopt three basic assumptions.

> ...that a forager encounters available prey types 'at random'—that is, in the same relative proportions throughout the foraging area... it is assumed that foragers search for all prey items simultaneously and that once a prey item has been encountered and the decision to pursue has been made the time spent handling it is unavailable for searching. Third, the forager is assumed to rank prey types along a

> single dimension of profitability, measured as net energy or food value obtained per unit handling time (Smith 1983:628).

Departures from these assumptions in actual situations will naturally result in a requirement to identify and factor in those differences if the the model output is to retain its high predictive value.

Additionally, Eric Smith reviews some important predictive outcomes of the fine-grained diet-breadth optimization model.

> First, as the availability of high-ranked prey types fluctuates, the optimal diet breadth shrinks or expands: increased availability should result in more specialized diets, while scarcity of high-ranked items favors a more generalized diet. ('Availability' refers here to *encounter rate*, which may often differ from actual environmental abundance.) Second, prey types should be added to or dropped from the diet in rank order of handling efficiency, with low-ranked types moving in and out of the forager's 'consumption mix' while the higher-ranked types are invariably pursued when encountered. Third, and least obvious, the inclusion of a prey type in the diet should depend only on the availability of higher-ranked prey types, *not* on its own availability... Conversely, high-ranked types should be harvested even if they are rarely encountered, as long as the expected return from handling them is higher than the expected return if they are passed by in favor of different prey. Thus, the *rank* of a prey type predicts not how common it will be in a forager's diet, but only whether it will be taken when encountered; to predict the number of prey harvested, we need to specify the encounter rates of prey types as well as their rank orders. It follows that we cannot use the commonness or rarity of a prey type in a forager's diet to predict whether it will remain in the diet under altered conditions: one prey type may be highly ranked but rarely encountered and thus rarely harvested (and thus seemingly 'unimportant'), while a staple that is abundant but ranked low in handling efficiency may suddenly be dropped from the diet if higher-ranked resources increase in availability (Smith 1983:628) [emphasis in original].

Additionally, and in the way of a summary, we may note that Tp values for particular prey types can be influenced in several ways. The relative size, strength, speed, physiology, and other biologically intrinsic characteristics of the forager relative to such traits exhibited by individual prey types can have a profound effect on pursuit and handling times.

Prey types which are (relative to the forager) considerably larger, more powerful, faster, or endowed with various effective defense mechanisms (including cooperative defense behaviors), will exact a greater investment of time and energy from a forager than organisms lacking or showing these same traits in decreasing amounts. Injuries to the forager incurred as part of pursuit, capture, and processing of prey must also be taken into account in acquiring specific prey or prey types. Furthermore, small organisms which can easily be overpowered by a forager may be "costly" in terms of their relative net energy return values.

In the case of very large prey types which can not be processed and consumed within a short period of time, the Tp (pursuit and handling time) factors incur some additional costs which may not be immediately obvious. Any portion of the prey which becomes unavailable to the forager due to spoilage or loss to predators or scavengers can not be validly considered as energy gains realized from that prey. Furthermore, any additional work expended on preservation or defense from predators and scavengers must be added to the overall (time/energy) costs associated with pursuit and handling time for that prey. Accurately assessing the actual Tp values of individual prey types leads to an accurate hierarchical arrangement of their relative net energy returns.

Search time (Ts values) are influenced, in turn, by a variety of other factors relating to specific prey types. The relative abundance of any one type of prey within an area is inversely proportional to search time. Numerically abundant prey are encountered more frequently under fine-grained conditions. This factor is conditioned in part by the forager's capacity to see and identify prey types. Camouflage by prey species or environmental conditions such as plant cover or an irregular topography can significantly decrease encounter rates and increase Ts values. Similarly, prey type recognition can also be influenced by the nature of past encounters between a forager and a particular prey type, rendering it familiar, uncertain, or unknown as prey.

Increased environmental productivity generally results in increased prey densities, resulting in a decrease in Ts values. Increased competition from other predators, on the other hand, tends to decrease the density of prey, resulting in increases in search times.

It is clear from all of these considerations that the relative ranking of prey types by net energy returns is anything but immediately or intuitively obvious. Any given ranked list of prey types may very well turn out to be quite heterogeneous in composition. Furthermore, despite the requirement that we approach prediction through a process which requires the ranking of prey species according to net energy return (Ts + Tp) values for each prey type, it is quite possible that fair numbers of prey types in such a list may exhibit (Ts + Tp) values which are extremely similar (despite the fact that such a group is quite heterogeneous on the basis of other criteria). Ts values, being density-dependent, can result in fluctuating (Ts + Tp) values over space and time.

From the point of view of prey ranking, any prey types which exhibit very similar (Ts + Tp) values, or fluctuating value ranges will yield effectively no inherent discernible differences between prey types. Broadening diet breadth negates the need for a ranking system within such a group.

It is not clear how often such a group of closely-ranked organisms is likely to become a significant factor in the diet-breadth of various foragers; however, several factors can promote such a situation.

First, it is more likely to occur with foragers which can anatomically, behaviorally, and physiologically access a wider range of prey types. Second, it is more likely that an environment characterized by a wide variety of species will yield close-ranked prey sets than an environment characterized by low or poor species diversity. Finally, since highly productive environments are typically associated with high species diversity, while environments with low productivity tend to have relatively poor species diversity, close-ranked prey sets may appear more frequently in environments which are highly productive than in environments of low productivity.

Under optimum combinations of such variables, increasing close-ranked prey sets not only widens the diet-breadth of a forager but less obviously promotes the maintenance of such ranked sets. By viewing several prey types as equally or closely ranked, the relative abundance of each such prey type can be added to the abundance of each of the other prey types in a set. This composite abundance value greatly increases the likelihood of encountering one or another prey type from such a set, thus decreasing the search time (Ts values) associated with the whole set. Since the ranking of prey types is dependent on (Ts + Tp) combined values, the ranking of the whole set (relative to other individual prey types) will rise, increasing their preferability over other prey types. Under such conditions, the net energy return criterion of optimal foraging will favor a wide diet-breadth adaptation.

Theoretically, it may be argued that the anatomical, behavioral and physiological traits which characterize biological "generalists" (such as Ursus americanus, an omnivore) will predispose them to ranking various potential prey into ranked sets, resulting in wide diet-breadths for biological generalists. Conversely, biological specialists (e.g., Puma concolor, an exclusive carnivore) with very specific foraging anatomical equipment, narrow physiological tolerances for food composition, and specialized foraging behaviors will more clearly rank prey types according to net energy return differences . This, in turn, will retard or prevent the appearance of ranked sets; and will, therefore, predispose such organisms to a narrower, more specialized diet. Given these intrinsic biological predispositions, it can be predicted that biological generalists will attain their highest population densities in environments which are highly productive and contain a wide variety of potential prey species.

3.2 SMALL-SCALE SOCIETIES: INDIVIDUAL VERSUS SYSTEMIC APPROACHES TO FORAGING

From the standpoint of foraging behaviors, I wish to propose a basic dichotomy in the ways in which individuals composing small-scale societies forage in given environments; namely, human foraging can be classified as either individual foraging or systemic foraging. People living in small-scale societies use both types of foraging, but the relative proportion and quality of the contribution of each foraging mode varies greatly between individual societies. Collectively, the contribution of these two modes of foraging constitute the total energy "income" of small-scale societies.

It is critical to recognize at this point that, based on the principles of optimal foraging discussed earlier, the relative ranking of prey types is dependent on the pursuit and handling times (Tp), and on the search time (Ts) associated with each prey type. These, in turn, are not absolute values; and typically must be calculated, in part, relative to the size and other characteristics of the forager.

Individual human foragers working in isolation and encountering prey at random will be faced with a different set of rankings of prey from those which might characterize situations of deliberate systemic (often cooperative) foraging. These differences in prey ranking will express themselves in different forager-to-prey interactions. Differences in prey rankings will exhibit themselves as differences in the (Tp + Ts) values, allowing individual foragers to choose between two sets of potentially quite different optimally-ranked prey types.

Human biology, including anatomical and physiological traits, predispose our species to fit best a niche corresponding to that of a generalized omnivorous forager. Associated capacities for resolving complex problems, tool making, and tool use (as modified or unmodified natural objects) enable human individuals to access directly a wide variety of plant and animal food sources. Optimal foraging theory predicts that in these situations the forager will pursue and harvest high-ranked prey types.

Often, however, the ranking of these prey types would shift dramatically if the forager were to search systematically for these types of prey (increasing search costs), or if the forager were to carry or make on the spot some type of device which allowed prey to be carried back for consumption at a central place (increased processing and transport costs). In these situations, the greatest net energy returns would be realized by immediate consumption of the prey by the individual forager. These observations lead to the conclusion that members of small-scale societies may, when foraging alone, eat what they find to satisfy immediate needs.

Individual foraging does not negate the value or (frequently) the necessity for systemic foraging; although being relatively low in associated direct search and pursuit and handling costs, individual foraging is very high in net energy returns; and as such, it is a significant potential contributor to the overall energy budget of a small-scale society. The inclusion of individual foraging in the overall foraging strategy, even in situations in which systemic foraging is clearly the principal foraging objective, can be viewed as a means of decreasing the overall energy costs associated with the search, pursuit, and handling of systemic prey types.

Individual foraging temporarily subordinates the wider needs of the cultural system to the immediate needs of the individual. This subordination of systemic needs may appear to be a pattern of behavior which disrupts systemic integration; however, from a wider perspective, this is not the case at all. Given the low number of individuals which typically characterize individual small-scale societies, and given the cultural system's reliance, and in fact, total dependence on individual labor, participation, and biomass (as a source of energy reserves), individual foraging fosters systemic homeostasis by allowing access to energy sources with high net energy returns which ultimately benefit the system as a whole.

Attention to individual foraging, as a recognizable and legitimate category of human foraging behavior, is largely lacking in the anthropological literature, although indirect references to such behavior are not difficult to find. Some authors like Silberbauer (1972; 1981), Hayden (1981), and Mann (1981) have made more explicit references to examples in the ethnographic literature of what they refer to as "snacks," or "the snack factor."

Often the ethnographic mention of foraging behaviors which might be classified as individual foraging takes the form of indirect comments. In describing the foraging behaviors of Washo men, James Downs states that "except for the pine nut and those foods taken in small amount and eaten on the spot, all gathering was done by the women" (Downs 1966:21). In another section, Downs notes significantly that

> Fresh birds swelled the diet and reduced the pressure on stored food such as pine nut flour and dried deer meat. Birds appeared to be considered what in modern military language we would call a 'target of opportunity.' That is, very little general planning went into bird hunting. If they were present, they were taken, but seldom do Washo informants speak of shifting camp or making special long trips to hunt either waterfowl or upland game birds (Downs 1966:34).

Norman Tindale is more explicit in his observations of the plant-gathering behaviors of the Pitjandjara of the Australian Western Desert. "Men gather to eat native fruits and search for vegetable food for their own immediate needs while walking out to stalk game, but they neither gather it for their families nor carry such products back to camp" (Tindale 1972:248). Tindale goes on to state that

> The role of children in food-gathering starts as soon as they can sit up and take notice. By the time they are ten years old they are capable of satisfying perhaps as much as a fourth of their daily requirements while following in the wake of their mother's wanderings in search of vegetable food (Tindale 1972:249).

In a discussion of male hunting activities among the Hadza, Woodburn points out that

> Men hunt individually and decide for themselves where and when they will go hunting. When a man goes off into the bush with his bow and arrows, his main interest is usually to satisfy his hunger. Once he has satisfied his hunger by eating berries or by shooting and catching some small animal, he is unlikely to make much effort to shoot a large animal. Of course, if he sees an animal close by which can easily be hunted, he will almost always take the opportunity. Men most often return from the bush empty-handed but

> with their hunger satisfied (Woodburn 1968:53).

Speaking of the G/wi Bushmen, Silberbauer notes that in times of relative plenty "...men gather only snacks for themselves while out hunting for the day, or for their meals while traveling" (Silberbauer 1981:468; see also Silberbauer 1972:288). Elsewhere, however, he notes that "no estimate was made of the numbers of small birds and reptiles taken, as these are seldom brought back to camp (except in times of great food shortage), but are eaten as a snack out in the veldt, and many are taken by children" (Silberbauer 1972:284). It is precisely observations of this type which prompted Brian Hayden to note parenthetically that "it is remarkable how often the statement appears [in the ethnographic literature] that natives ate 'undetermined' amounts of food as snacks while foraging" (Hayden 1981:344).

Often the gathering and immediate consumption of foods is viewed as a function of the specialized nature of some types of food, such as certain types of insects or larvae. Pierre Clastres notes as part of the foraging behavior of the Guayaki of Paraguay that upon encountering various larvae "...they are eaten as soon as they are found" (Clastres 1972:160); while elsewhere he notes that "there is no apparent order in the hour of the Indian meal. One eats when one is hungry. It seems, however, that the evening meal is the most consistent of the day" (Clastres (1972:151).

References to such individualized and casual forms of foraging are sometimes more oblique, as when Peter Gardner notes of the Paliyans of south India that

> Although several food-collecting activities benefit from some degree of cooperation between families, the majority of subsistence pursuits entail only individuals or at most nuclear families. Without much hardship at all, one person alone may constitute a unit of production and consumption (Gardner 1972:415).

At other times, however, such references are much more explicit. Reviewing the ethnographic accounts of the Tasaday, Alan Mann points out that "...the Tasaday fill about 78% of their protein needs but only about 27% of their energy needs from meals eaten at camp; much of the rest is presumably eaten during the day, as individuals immediately consume food discovered during their daily rounds" (Mann 1981:12).

In view of the preceding statements, together with other similar statements in the ethnographic literature which can be ascribed to what I have labeled as "individual foraging," Hayden concludes that

> Eating while foraging is much more prevalent among generalized hunter/gatherers than has usually been assumed. This probably has had an important skewing effect on most assessments of nutritional adequacy and food preferences in the literature (Hayden 1981:419).

In reviewing these references to individual foraging, I wish to emphasize that I view such behavior as a supplement rather than as an alternative to the more traditionally recognized forms of specialized, planned, cooperative, systemic forms of foraging (tied to food sharing and division of labor) normally associated with typical descriptions of foraging behaviors in small-scale societies (eg., hunting of larger game among the Mbuti [Turnbull 1961] or the !Kung [Lee 1979]; or even conducting rabbit drives or collecting pinyon nuts by the Washo [Downs 1966]). It is quite clear that in many if not most cases, the bulk of the food, including energy, protein, and nutrient requirements, is supplied through such systemic forms of foraging and food sharing. Additionally, systemic foraging strategies, utilizing a central place at which food distribution and processing activities can take place, are also tied to and make possible a wide range of other systemic behaviors designed to meet social and reproductive requirements, to promote the flow of information between individuals, and to provide for collective defense from predators while engaging in non-foraging activities.

All of these represent significant systemically-generated benefits associated with systemic foraging, which supports individual and group survival while promoting systemic homeostasis and continuity. However, none of these outcomes negates the relatively large net energy returns and associated benefits accrued by the inclusion of individual foraging behaviors in the overall foraging repertoire of small-scale societies. Thus, the critical issues relating to individual versus systemic forms of foraging depend more directly on determining the most appropriate mix or balance of these two forms of foraging in the overall foraging behaviors which characterize each small-scale society within the context of specific environments and other factors such as population densities and non-foraging requirements.

Individual foraging in small-scale societies is based on the fine-grained diet-breadth model of optimal foraging theory. This approach, in turn, assumes that individual human foragers will encounter fine-grained distributions of prey types—an assumption which needs to be examined more closely in light of the fact that many environments would appear at first glance to be patchy or coarse-grained, rather than fine-grained.

It must be remembered that environments can be classified as fine-grained or coarse-grained on the basis of the spatial distribution of various living organisms. Furthermore, each individual organismic type often has its own distinctive distribution pattern. Viewed from the perspective of an individual organism, larger body size requires greater absolute amounts of energy to sustain normal life functions. As a result, larger individual body size is often associated with a more coarse-grained or patchy distribution. Smaller body size, on the other hand, requires less energy to support; and this factor in turn can result in a fine-grained distribution pattern.

Small size and resultant vulnerability have some further significant outcomes. Small-sized prey are individually much less mobile (they are more sessile) than large prey species. At the same time, their vulnerability to predators, together with a relatively short life span, requires that they produce fast-growing offspring in relatively large numbers and at frequent intervals. In many ways, then, this set of traits mirror the traits associated with r-selected species (for a discussion of r-strategists and K-strategists see Smith 1986:273-74).

Since environmental graininess (as seen in modern ecosystems) is tied to distributional patterns which characterize individual organismic types or species, a given environment may be characterized as fine-grained for one type of organismic distribution, while being coarse-grained for other types of organisms. This situation is the key to understanding some of the crucial differences in human individual versus systemic foraging strategies.

Individual foraging in small-scale societies is oriented toward small prey types with relatively fine-grained and widespread distribution, accessed most directly (organismically) by individual human foragers with little or no need for planning or systemic devices needed to access them. This is precisely the approach to foraging which Downs (1966, above) has characterized as "target of opportunity." Widespread, regular availability of such prey types associated with low search and pursuit and handling costs followed by immediate consumption forms the key to understanding the high net energy returns associated with such an approach to foraging. Only in situations in which the distribution or the associated handling costs change in some significant way are such prey types likely to be approached from a systemic foraging mode.

Despite small prey size, seasonal patchy concentrations are likely to be accessed through systemic foraging strategies. This appears to be the case for a wide range of prey types (plant as well as animal) including the harvesting of acorns among California Indians (cf. Gould 1981); pine nuts among the Washo in the Great Basin (cf. Downs 1966); or even grasshoppers among some other Great Basin Indians (cf. Madsen and Kirkman 1988).

The concentration of resources in space, or both time and space, results in the introduction of added factors which influence net energy returns to a forager. The collection and use of information about the availability of concentrations of prey takes on a key role in systemic foraging which is directly related to the search time costs (Ts) associated with specific prey types. Larger prey sizes are frequently accompanied by a more patchy distribution requiring increased travel time costs. Furthermore, there is often a wide variety of new pursuit and handling costs (Tp) incurred through other associated factors.

With some exceptions such as some species of tortoises, most larger prey are more mobile (vagile) and can therefore more easily elude capture. Larger individual prey types also represent a different forager-to-prey size ratio, making it easier for prey to defend themselves. Concentrations of smaller prey (such as acorns) often require energy expenditures due to some other associated factors. Acorns need to be ground and leached, and Gould notes that "...the nuts must be collected fairly fast, since they may be infested by weevils if left on the ground for long" (Gould 1981:442). The very existence of concentrations of prey in time and space may often also mean increases in competition from other species, and potential new dangers associated with increased concentrations of predators. All of these factors produce significant increases in pursuit and handling costs.

Unlike the fine-grained diet-breadth model, optimization models which address foraging strategies in coarse-grained or patchy environments include a larger number of variable environmental factors requiring the generation of multiple alternative model outputs—each geared to address specific sets of environmental conditions. No single model is available which can simultaneously address all of the potentially variable sets of environmental conditions which may be encountered, although each alternative model output can be seen as some type of extension of the principles which underlie the fine-grained diet-breadth model.

Optimization in coarse-grained or patchy environments produces different model outputs in which certain variables are specified or fixed, while other variables are allowed to vary. A forager's choice of patches can be optimized by ranking patches by their net energy returns, and otherwise substituting patches for prey types in the manner discussed for the diet-breadth graph shown in figure 3.1 (above). Within this context, three possible approaches to foraging can be predicted under three separate sets of assumptions.

Under conditions in which prey are highly visible and sessile, a forager may continue foraging in a patch until all prey have been exhausted. If a patch contains resources which replenish themselves at a rate which equals the rate of resource depletion caused by foraging, then the optimal foraging strategy consists of choosing the most productive such patch and remaining within that patch as long as conditions do not change. Neither of these alternatives appears to be very common or realistic.

> A third and theoretically more interesting set of conditions is found when foraging within a patch causes a gradual but continual decline in the return rate for that patch. This case—the classic economic situation of 'diminishing returns' to inputs of time or labor—is treated by a foraging model termed the marginal value theorem... It makes the explicit assumption that the foraging process gradually depletes the resource level in any patch and that this causes a monotonic decline in the net return rate from that patch (e.g., as prey become harder to locate). The critical variable to be optimized is hence the point of this curve of diminishing returns at which the forager should depart for another patch. Assuming that the foraging period in any one patch is short relative to renewal time, the optimal solution is to leave each patch when the marginal capture rate (i.e., the instantaneous capture rate at the end of a foraging period within that patch) is equal to the overall mean capture rate (averaged over the entire set of patches utilized, including travel time between patches)...
>
> Several predictions follow from this model. First, the optimal forager should leave any patch when it is depleted to the point where foraging elsewhere will yield higher returns per unit time (taking travel costs into account). Second, since optimal time allocation to any patch is a

> function of average yields for all utilized patches, as overall productivity of a habitat (set of patches) rises less time should be spent in any one patch and conversely (declining productivity increases optimal patch-stay times). A similar prediction holds with changes in travel costs, with lower costs being equivalent to higher productivity and conversely. Finally, any patch not yet in the utilized set should not be added unless it can yield a marginal rate of return equal to or greater than the average rate for the utilized set (Smith 1983:631).

3.3 SMALL-SCALE SOCIETIES: QUESTIONS OF ENVIRONMENTAL AWARENESS, AND FORAGING CHOICES

At this point it would be particularly useful to make a critical distinction between the theoretical principles and predictions of optimal foraging theory, which within the confines of the stated variables are difficult to challenge; and their application to actual foraging situations. One of the "hidden" assumptions of some optimal foraging theory constructs is that foragers operate on the basis of perfect environmental knowledge or awareness, and exact or perfect capacities to calculate cost-benefit ratios associated with alternative foraging behaviors. These conditions are clearly impossible to meet in actual foraging situations. Based on these conditions, two additional outcomes must be specified.

First, since the theoretical preconditions of perfect environmental knowledge (awareness), and perfect capacity to calculate cost-benefit ratios will never be attained in actual situations, it should follow, then, that actual optimization by foragers will always represent some suboptimal value relative to the model output's predictions.

Second, since optimal foraging theory is tied to organismic fitness values and the process of natural selection, it follows that the relative fitness of a forager will be a function of the degree to which its suboptimal environmental knowledge and capacity to calculate cost-benefit ratios approaches the optimal values predicted by the model output. If this is so, and other factors are held constant, we may predict that the difference in the capacities of two or more competing foragers or groups of foragers with different levels of environmental awareness and capacities to calculate cost-benefit ratios of their foraging alternatives will express itself as differential reproductive success over time.

In other words, in the absence of other intervening variables, we should be able to predict that capacities and behaviors which increase a forager's environmental knowledge and abilities to optimize foraging choices will be actively selected for. This is an especially provocative conclusion in light of both the evolution of human reasoning and decision-making capacities on the one hand, and the development of systemic foraging patterns in human small-scale societies. In effect, it could be argued that both of these characteristics might have been under active selection as a result of the differential fitness conferred on organisms or groups of organisms exhibiting improved suboptimal capacities as previously specified.

In addition to the variety of factors which are dealt with by classic models of optimal foraging in patchy environments, human foragers approach the minimizing of associated (Ts + Tp) costs through a variety of systemic cultural approaches. Perhaps the most cost-effective approach utilized by human foragers involves the collection, sharing, cognitive modeling, and transgenerational transmission of environmental information. In addition, human systemic foraging includes the use of technological solutions, and cooperative, group foraging. These approaches, in turn, become viable by the use of central place foraging and by sharing resources.

From a systemic point of view, systemic homeostasis and continuity are promoted not by optimizing individual foraging, systemic foraging, technology, information systems, reproductive behaviors, or cooperative social behaviors singly, but rather by developing an optimizing mix which balances all these factors. Under these conditions it is quite likely that all of the component factors will be compromised to some degree so that the "optimum" values obtained will be equivalent to some suboptimal level compared to theoretical predictions, the goal of which is to predict the optimum for any one of these factors.

From a systemic point of view, all of these factors are mutually interdependent. Systemic foraging in small-scale societies does not function in isolation from individual foraging or from information systems or reproductive behaviors.

Systemic foraging allows small-scale societies effectively to access additional prey types and larger volumes of food biomass which exceed the potentials of individual foraging alone. This factor results in increased population numbers and population densities which could not be supported through individual foraging. Increased population densities in turn facilitate group defense and increase the availability of potential mates, supporting social and reproductive requirements.

Individual foraging, with its high net energy return values, acts to minimize the costs associated with environmental information gathering, and with travel time between patches or prey. The information which is gathered in this way in turn becomes useful through social information sharing in the context of systemic central place foraging. In this sense, systemic foraging, individual foraging, and information systems are bound to each other in a tight network of relationships. Technological solutions employed in effecting prey capture through individual and systemic foraging alternatives are frequently tied to broader environmental information.

Although some types of individual foraging behaviors are facilitated to some extent by the availability of technological solutions, most technological foraging solutions are associated with systemic forms of foraging behaviors. By definition, individual foraging yields high net energy returns only because it does not require the implementation of capacities beyond those which are already present in individuals as biological organisms. Focusing, as it does, on relatively small prey scattered in a fine-grained pattern, the potential increased returns relative to the (Tp) energy costs associated with technological solutions would serve only to decrease the net energy returns realized. Thus, the likelihood and relative contribution of individual foraging to the total energy income of a small-scale society is more closely tied to the availability of suitable prey types, their densities, and the characteristics of the systemic foraging options employed by the society, than to the availability of technological solutions geared to solving individual foraging requirements.

Often the relative contribution of individual foraging to the overall energy income of a small-scale society is likely to be lower than the contribution of systemic foraging, if only because the density of suitable prey types in most environments would yield insufficient net energy gains to support sufficient population densities needed to maintain reproductive and other culture systemic requirements. On the other hand, including systemic approaches to foraging will inevitably increase (Ts + Tp) costs, increasing the overall demand for energy flow—a demand which must be met through increased gains in net energy receipts realized through systemic foraging behaviors. Thus, the costs and gains realized through systemic foraging must be balanced in relation to individual foraging to yield a net positive gain.

Because systemic foraging alternatives constitute both energy costs and energy gains, optimization of net energy returns from these alternatives will involve a variety of solutions. Some of the solutions are predicted by optimal foraging theory as it applies to patchy environments, and particularly by the predictions of the marginal value theorem. Other solutions will result from the development and use of appropriate cognitive model outputs (i.e., culturally acceptable prey choices, or organizational strategies for accessing a set of prey types), foraging or prey capture behavioral choices (such as specific hunting or gathering techniques), and associated technological options. Yet both of these types of solutions will depend upon the gathering and sharing of appropriate information.

Because individual foraging usually yields a higher net energy return, inclusion of individual foraging in the overall foraging strategy of a small-scale society raises the average net return of all foraging behavior. This net gain, in turn, can be used to cover various systemic costs—though from a foraging point of view, this gain can most effectively be invested into information gathering and covering travel costs between patches during systemic foraging. Viewed in this light, the net energy gains realized through individual foraging behaviors becomes an integral means to insure the maximization of energy returns realized through systemic foraging.

Based on these relationships it becomes possible to predict that (a) whenever possible, individual foraging behaviors will constitute part of the total set of foraging behaviors exhibited by members of small-scale societies; (b) individual foraging behaviors will be highly unlikely to constitute the exclusive approach to foraging employed in any small-scale society; (c) given the associated high net energy returns, individual foraging (relative to systemic foraging) will tend to be maximized whenever possible; and (d) the proportion of individual to systemic forms of foraging will be a function in part of four principal components: 1) the availability and density of suitable prey accessible through each type of foraging; 2) the relative energy returns of each type of foraging behavior compared to the average rate of net energy returns; 3] the technological solutions available systemically to access alternative prey types; and 4] the total systemic energy requirements of the small-scale society.

The critical dimension of the above-stated predictions on which I wish to focus is the relative proportion of the individual to systemic foraging characterizing the total energy budget income of a small-scale society. The first two components of prediction "d" [above] relate to environmental parameters; while the last two deal with cultural components.

Given prediction "c" [above], the assessment of environmental characteristics should begin with an evaluation of the relative suitability of specific environments to support individual foraging modes. Systemic foraging modes are therefore viewed as additional sources of food energy employed by small-scale societies, above and beyond the net energy gains realized through individual foraging. This distinction is based on the relative difference in net energy returns of each foraging strategy; and it points to a means of relative prioritization, rather than a dismissal or a way of discounting the universally significant contribution of systemic foraging modes to the subsistence strategies and total energy income of small-scale societies.

The categorization of prey types available through individual foraging relates most directly to innate human physical and physiological capacities to capture and consume individual prey types without prior planning or searching [not excluding experiential capacities to recognize prey and prey habits, nor excluding limited energy investments in pursuit and capture], and with little or no use of technological or systemic aids.

The "availability" of suitable prey types for capture through systemic foraging modes, however, is directly tied to various culture systemic factors including the specific content of cultural information systems [including technological capacities], social systemic energy requirements, social systemic energy reserves, and biocultural systemic capacities to profit from "pulsed" increases in energy flow relative to energy expended in systemic prey capture. The mere presence or existence of certain prey types in a habitat, without prior reference to specific aspects of the energy budget and dynamics of a cultural system, can not be automatically equated with accessibility or "availability" of those prey for use through systemic foraging.

The categorization of prey types as accessible through individual as opposed to systemic foraging in the absence of other considerations can also lead to errors. Although the characteristics of particular prey types [viewed individually] are an important factor, their distribution in time and space are equally important. Thus, for example, one might be tempted to categorize grasshoppers as prey which might be typically accessed through individual foraging, yet in some situations, grasshoppers might be prey types suitable for access through systemic foraging [cf. Madsen and Kirkman 1988].

The success of individual foraging assumes the availability of various [relatively sessile] small prey types distributed in a fine-grained pattern over the foraging territory, insuring frequent and unplanned forager-prey encounters in which the forager can easily and fairly regularly access small quantities of food biomass. Recurrent concentrations of small prey types in time and space make possible the adoption of planning, cooperative foraging, and the use of technological aids in maximizing the rate of success in harvesting such prey. Under these conditions the same prey type which might otherwise be accessed through individual foraging modes will now be accessed through systemic foraging behaviors.

The operation of a number of such component variables in determining the "availability" and means of accessing (through individual or systemic foraging) of prey types has consequences in relation to niche width. Niche width for both types of foraging is dependent on information relating to specific prey types. Prey "availability" accessed through systemic foraging is further constrained by capacities to invest systemic energy in information, technology, and (at times) cooperative group foraging. Since these investments are often (in both obvious and more subtle ways) tied to particular prey types, systemic investments of energy will increase with each prey type accessed through systemic foraging.

If one were generally to predict niche width in the absence of specific and detailed culture-environmental information, then one might predict that given small local group sizes (perhaps of 25 or fewer individuals) having intimate and current environmental information, the niche width of prey types accessed through individual foraging modes will be relatively wide, while the niche width of prey types accessed through systemic foraging will be relatively narrow and confined to prey types which will afford the greatest net energy return values (i.e., as predicted in optimal foraging theory). Furthermore, such a group is likely to rely more heavily on individual foraging modes, with less emphasis placed on systemically accessed prey types.

On the other hand increased group size and forager population density—especially when coupled with the absolute availability or existence of potential prey types which could be accessed systemically—will result in decreased emphasis on prey types accessed through individual foraging modes (resulting in a narrowing of niche width for smaller prey types), while increasing the niche width of prey types accessed through systemic foraging modes. This situation is further enhanced by greater environmental productivity, and especially by greater productivity leading to overall increases in prey numbers and types available (in an absolute sense) through systemic foraging modes. Critical to these shifts, however, is the existence of increased group size and forager population densities.

Decreased absolute environmental productivity, on the other hand, exerts a limiting effect on forager population densities and foraging group size. Under these conditions, individual foraging will be maximized wherever possible, while the niche width of prey types accessed through systemic foraging modes will also increase—even if only to access a greater number of small prey types systemically which otherwise might have only been accessed through individual foraging modes. Larger, more vagile prey will still be accessed systemically, though the decrease in their total numbers will mean that the frequency of such foraging (and therefore their relative contribution to the overall diet) will significantly decrease as search and pursuit and handling time costs increase.

The impact of group size (having intimate, current environmental knowledge) on prey choices is documented ethnographically by the work of Beckerman and Sussenbach (1983) among a dozen groups of lowland northwestern South American Indians. At the conclusion of a lengthy and detailed set of comparisons of a dozen groups in various environments, they state that they failed to demonstrate any single, simple significant correlation.

> However, if we group the focal animals into size classes, a rough pattern suggests itself. Concentrating only on those societies which inhabit their villages for a year or more on the average, one can offer as a hypothesis for further testing that peoples who habitually live in groups of 50 or more will tend to focus on large animals (peccary, tapir, deer, or if nearby a large river, caiman) almost to the exclusion of the smaller species in their top five prey species, while peoples who habitually live in groups of 10 to 30 will tend to include substantially more smaller species such as paca, agouti, monkeys, and birds (Beckerman and Sussenbach 1983:344, 346).

Similarly, the pattern of prey choice and utilization relative to environmental productivity and group size can be documented by the ethnographic comparisons of subsistence patterns found among western Australian aborigines and native Californian Indian populations provided by Gould (1981). Whereas neither of these sources gives absolute ethnographic support to the theoretical predictions presented thus far, these ethnographic examples are cited here not only because they tend to support the predictions tendered above, but also because they give a more concrete form to an otherwise relatively abstract set of principles.

Simply viewing environments as being "permissive" or "restrictive" in their own right, without incorporating systemic and demographic variables into such definitions, tends to become misleading in all but the most extreme cases (cf. Bicchieri 1969). A more revealing perspective is presented in a recent set of comments by Bicchieri in which he states:

> That the bow hunting Hadza hit their target once in ten attempts... for example, leads to the Eurocentric conclusion that the Hadza are inept and inefficient. In contextualized adaptive terms, however, given that the Hadza have successfully integrated into their specific ecosystem for millennia, it would be more appropriate and analytically productive to see their inaccuracy as an indication of affluence. On the other hand, circumpolar survival in a very restrictive physical environment is predicated on close to a 100% kill success. Such a low allowable coefficient of failure implies a high degree of absolute efficiency but a relatively low level of affluence. One must be relatively wealthy

> in order to afford an absolute loss in resource acquisition and relatively poor not to be able to afford even minimal absolute losses (Bicchieri 1990:5-6).

In terms of the current discussion, the contrasts in "affluence" and the related degrees of relative systemic foraging efficiency can be understood as differences in the relative availability and concomitant reliance on energy gained through individual foraging to each of the two contrasted situations.

The Hadza can be seen as relatively "affluent" (and therefore capable of defining their environment as "permissive") because of their proportionately larger reliance on energy from individual foraging. Circumpolar survival, on the other hand, is associated with very low availability of energy derived from individual foraging modes, and high reliance on systemic foraging behaviors.

Given the increased energy costs associated with greater (Ts + Tp) costs in systemic foraging, it is not surprising that such environments can be defined as "restrictive," or characterized by low "affluence." In view of the context provided by the preceding theoretical considerations, "restrictive" and "permissive" environments can best be defined in conjunction with culture systemic factors, which in turn lead one to considerations of the relative reliance of a small-scale society on individual versus systemic foraging energy sources.

Returning to the discussion of theoretical predictions, the fine-grained, sessile distribution and relatively rapid reproductive rate of small prey types means that their availability through individual foraging behaviors attains a relatively constant value over large areas, although high predation rates by concentrations of foragers within localized areas can result in short-lived depletions of small prey, resulting in decreasing encounters and a diminishing net energy gain from individual foraging to the overall energy budget of a small-scale society. Assuming that the central place used for foraging is relocated frequently and before small prey types are severely reduced, a given habitat can be viewed as contributing a fairly constant rate of net energy gain through individual foraging.

If human foraging were limited theoretically to individual foraging, the relatively constant availability of small prey types could result in a fairly constant forager to small-prey ratio; that is, a carrying capacity equilibrium level. However, the group systemic strategies of human survival require spatial and temporal concentrations of human foragers which can not be supported strictly through individual foraging.

Systemic forms of foraging must be employed to augment the energy gains realized through individual foraging, both due to environmental factors (uneven or insufficient access to resources by single individuals equalized systemically), and due to inherent sexual and ontogenic differences of individuals (such as varying capacities to access specific prey types by men, women, pregnant or lactating women, children, or the elderly). Both of these types of factors result in major increases in fitness associated with group systemic foraging behaviors.

The greater the spatio-temporal disbalance between the energy available through individual foraging and the total energy requirements of a small-scale society, the larger the requirement will be for energy obtained through systemic foraging. These relationships, in turn, set two limiting factors on population growth.

The first limit relates to the overall group size which can forage from a central location. Increases in local group size will result in greater demands on small prey accessed through individual foraging, resulting in more rapid depletion and therefore rapidly escalating demands placed on systemic foraging, which must compensate for the decreased net energy receipts realized through individual foraging. The depletion of small prey types through individual foraging will be greatest at the point of central foraging, decreasing in all directions in inverse proportion to the distance from the central place. Since this is a short-term localized phenomenon, a drop in energy gains realized through individual foraging can be temporarily rectified through relocation of the central foraging place.

Both of these solutions (increased emphasis on systemic foraging and more frequent relocation of the central place) incur disproportional increases in systemic energy costs as the use of these strategies increases through time. As a result, a level of local population size (density) can be reached which can no longer be supported through increased systemic foraging or through more frequent relocations of the central place from which foraging takes place.

The absolute maximum size of such a local group will vary based on environmental parameters and the efficiency of the available systemic mechanisms (including technological devices) through which systemic foraging can access additional prey and still increase net energy returns. However, in all cases, diminishing net energy gains realized through these approaches will form a gradient which (thanks to increasing entropy costs) will increase disproportionately, creating increased levels of systemic stress ultimately requiring systemic reorganization. In combination, this becomes a range of acceptable maximum group size, a range which is often observed ethnographically as somewhere between 15 and 50 individuals (in most hunting and gathering societies), with the most typical or modal values centering around 25 to 30 individuals.

The second limit to population growth results from the combination of two factors related to the overall size and productivity of the territory which a small-scale society can regularly utilize for all types of foraging. The first of these factors is a function of the rate of small prey replenishment relative to the number of non-overlapping central foraging locations (including surrounding foraging zones) which can be utilized in some sequence. This sequence must insure that the group will be able to relocate regularly and indefinitely to a new central place where small game have fully re-established their numbers since the last time that the group occupied the area.

The second factor relates to the replacement rate of prey types accessed through systemic foraging. Many, though not all, of these prey types can be classified as K-selected prey which will be relatively slower to replace their numbers lost through predation than smaller prey types. Significant decreases in their density are particularly serious limiting factors on overall forager population densities, given the predictions of the marginal value theorem.

As with any other type of organism studied by ecologists, human forager densities are not a function of relative resource abundance, or resource availability during periods of relative abundance, but rather they are a function of the temporal incidence of periods of resource scarcity. The two interconnected modes of foraging may limit population densities especially during times when total energy availability from both modes of foraging is reduced at the same time. This is clearly the perspective which formed the theoretical basis for the conclusions reached in the ethnohistorical study of Algonkian subsistence presented by Waisberg (1975).

Finally, a variety of related environmental factors can also limit group size and overall population densities. Important among these is the availability of dependable water supplies, fuel (especially critical in colder environments), and suitable locations and materials for constructing various types of shelter. The criteria for defining acceptable shelter is probably the most variable of these three limiting factors, since a wide variety of cultural, climatic, biotic, and geophysical factors will interact (often in the context of microhabitats) to define what is acceptable.

The need for fuel (usually firewood) is more constant—even in tropical and subtropical areas. Speaking of the normal gathering activities performed by G/wi women in the Kalahari, Silberbauer notes:

> To the load of plant food must be added the required amount of firewood. At the end of the day a woman will come home with nearly the equal of her own weight piled on her shoulders and slung in her antelope-hide cloak, and less than half of this is food (Silberbauer 1981:469).

It seems clear, then, that in addition to various foraging related factors, a variety of other factors must also be considered in the size and location of settlements employed by small-scale societies.

3.4 SMALL-SCALE SOCIETIES: DEMOGRAPHIC FACTORS, INCLUDING GROWTH RATES

So far I have dealt primarily with small-scale societies from the point of view of energy optimization and alternative subsistence or foraging behaviors. As I have tried to indicate, alternative subsistence or foraging behaviors do not function in isolation from other aspects of the cultural system, or from human demographic factors. Energy budgets, division of labor, choices of prey, size of territories, age and sex composition of populations, and reproductive rates all interact with each other through various feedback relationships. The outcome of these relationships can be expressed demographically. Therefore, the subsequent discussions of demographic factors as they apply to small-scale societies builds on previous discussions, while opening new avenues to the analysis of cultural dynamics by quantitative means.

One of the fundamental components which any demographic model of small-scale (hunting and gathering) societies must address is rate of population growth. Population growth patterns interact with a wide range of critical factors including the availability of prey per individual forager, the choice or mix of individual versus systemic foraging utilized in subsistence, the size and composition of regional mating networks, and significant growth rates may even provide an important impetus for emigration through increased population pressure (cf. Smith 1972:8).

Population growth rates are dependent on the rates of births and deaths (including age and sex-specific deaths) which characterize the population. The total outcome of these variables in a population will yield a value which can be expressed (as a percent of the whole) as the annual growth rate (or rate of change through time) of a population. This rate is normally represented by the symbol r.

Since the annual growth rate (r) implies a rate of change in population numbers through time, the length of time (expressed in number of years and represented by the symbol (t) over which population growth takes place becomes another significant element in such calculations. These symbols and the entities and relationships which they describe are used here in their standard forms as described in most texts on demography (cf. Bogue 1969; or Weller and Bouvier 1981). Their relationships are used in reporting normal research results in the field (e.g., Coale 1974), while a more detailed yet concise overview of the pertinent formulae and the manner in which they can be used to calculate relevant values (as I intend to use them, below) can be found in Ehrlich and Ehrlich (1970:10).

When we consider modeling population growth rates (r) in small-scale societies (including small-scale societies in the past), we may begin by employing the raw data which characterizes the observed growth rates found among such populations in ethnographic examples. Such an approach advocated by some researchers (cf. Ward and Weiss 1976, or Howell 1976) requires minimally the assumption that the present is a valid source of data for modeling the past. As Nancy Howell (1976:26) states:

> A uniformitarian position in paleodemography implies that the human animal has not basically changed in its direct biological response to the environment in processes of ovulation, spermatogenesis, length of pregnancy, degree of helplessness of the young and rates of maturation and senility over time. This does not imply that humans have not changed in the rates of performance of these processes, but only that the processes still respond in the same way to variations in environment, including the cultural and technological aspects of human society as part of the external environment.... The limitations of this argument to Homo sapiens should be underlined. Earlier species of hominids may have matured at a different rate, may have had shorter pregnancies, had multiple births or more mature offspring at birth, or whatever. This argument does not apply to them [emphasis in original].

Thus, the extension of current ethnographic inductive generalizations into the past does not imply an equivalent rate of performance (i.e., population growth rates may be significantly altered despite biological continuity); and in the case of even greater temporal extensions of these generalizations, the very fabric of biological constants on which a "uniformitarian" paleodemographic perspective is founded might become hopelessly unreliable. Severe as these limitations might seem, there are additional problems to be considered in the application of ethnographic data toward the modeling of the paleodemography of small-scale societies.

Modern demographic studies of ethnographic small-scale hunters and gatherers are relatively few and recent. By comparison, standard demographic treatment of population data involves the study of statistical trends of large numbers of individuals, often studied over a succession of generations. The extant ethnographic data base is insufficient to meet these criteria.

Furthermore, modern demography and its methods arose as a result of the availability of widespread and reliable written records documenting births and deaths. Although ethnographic approaches applicable to the demographic study of small-scale societies employ the data gathering and record keeping skills of the ethnographer, much of the actual data gathered (by necessity) must exceed the data available for direct observation and recording. Alternatively, informants must be prompted to supply longitudinal data from memory—a practice which may introduce various errors, not the least of which might be the omission of stillbirths, infanticide, or infant deaths due to other causes during the first few months of the infant's life.

Differential kinship ties, variable group composition, and the loss of adults (acting as information repositories in small-scale societies) can further introduce sources of potential confusion and error into these studies. Even if we were to assume that ethnographic studies of the demography of small-scale societies could be relied on to provide reliable data free from the above-mentioned problems, other potential sources of problems need to be considered.

Aside from the on-going debate about the "marginality" of modern surviving small-scale societies, it should be obvious that most current surviving small-scale societies do not occupy the range of environments they must have occupied in the past. The environments occupied by small-scale societies in the past would have included some of the most ecologically productive areas, which, in most parts of the world, have been occupied for the last few thousands of years by larger, settled, agricultural societies. If demographic variables are influenced, even in part, by environmental factors, then the available current sample is likely to be skewed in some significant ways.

It is unnecessary to enumerate historic examples of the toll in human lives which frequently accompanied contact between larger, more complex populations and small-scale societies. Such contacts have often served to decimate the population of simpler societies as a result of active conflict, or disease. Additionally, the effects of acculturation on culture-environmental relationships should not be underestimated.

In the absence of written records or reliable historical accounts, and in view of the other associated uncertainties, it would be difficult to argue that current ethnographically-derived demographic generalizations represent either typical or atypical conditions for small-scale societies today, or in the past. Lacking definitive answers which could solve the potential uncertainties of this method, we are forced, reluctantly, to accept the results of current studies by default. There is, however, an approach to the study of the demography of small-scale societies which might serve as an additional source of insight and as a check to unrestricted inductive generalizations.

In 1977, demographer Colin Clark addressed the issue of population growth rates in small-scale societies as follows:

> A virtual absence, or very low rates of population increase are in fact generally observed by anthropologists among primitive peoples in the world now. To take one interesting example, Australians often reproach themselves with the fact that the full-blooded aboriginal population, still living a nomadic hunting life, appears to be stationary in numbers. It is true that the evidence for this is not very accurate. For many years the writer was State Statistician of Queensland, which has a substantial aboriginal population, and used to prepare reports showing that it was approximately stationary, based on the only available evidence, namely questionnaires sent to the police officers responsible for the very large semi-arid districts in which the aborigines lived. These police officers, in the course of their multifarious duties, nevertheless made quick estimates, which were probably the most accurate obtainable, which did in fact indicate a stationary population. But we do now have considerable other evidence to show that this is only what was to have been expected.
>
> Much more valid and comprehensive, though indirect, evidence can be obtained by 'jobbing back'. At the present time we see world population growing at nearly 2 per cent per year, and in some countries at 3 per cent or 4 per cent per year. Nothing approaching these rates prevailed in the past (Clark 1977:60-61).

Clark's meaning can be better understood when we approach the related issues from a broader statistical and temporal base. He is not speaking of better methods in collecting data, or more reliable data from ethnographic sources. His reference to "jobbing back," followed by his mention of the current observed growth rates in various countries and world growth rates, constitute integral parts of an alternative approach which has been widely utilized by various demographers.

In essence, Clark's calculations, and the similar calculations of other demographers (cf. Coale 1974; Weller and Bouvier 1981; or Ehrlich and Ehrlich 1970), are based on the combined use of historical documents and records for regional and world population numbers, together with mathematical calculations of population growth rates extending from the present into the distant past. From such a vantage point the current high rates of population growth (rates which we individually and culturally take for granted) gain an entirely new perspective.

> In the perspective of the thousands of centuries of man's previous existence on earth, the modern era must appear as one of unusually rapid growth. In earlier epochs, there probably were also periods of growth, alternating with periods of stagnation or even decline, but it is highly improbable that there has ever been an era of sustained world-wide growth such as that of the past 300 years, or a period of such rapid growth as occurred during the past 50 years. For, had mankind's numbers been continuously increasing as rapidly as they did from 1900 to 1950, the whole present world population could have descended from one human couple in only 24 centuries. Nevertheless, it is known that human beings have peopled the earth for thousands of centuries before recorded history began (Clark 1977:61).

Still, at least on the surface, Clark's statements appear to be almost anecdotal until we begin to apply some of the standard demographic formulas used in calculating population growth rates.

If we let the symbol No equal the population size at time zero, and Nt equal the population size at time t, then the ratio of population change (increase or decrease) from time zero to time t can be computed by dividing the value of Nt by the value of No. Following the example provided by Ehrlich and Ehrlich (1970:10), if a population has doubled in size during this elapsed time, then Nt/No = 2. The natural logarithm (ln) of the ratio of population change (Nt/No set at a value of 2, in this example) equals the product of rate (r) and time (t); that is, in this case, ln 2 = rt. This set of relationships can alternately be expressed by the formulae:

$$\frac{\ln 2}{r} = t \qquad \text{or} \qquad \frac{\ln 2}{t} = r$$

These equations allow us to solve either for the value of t (the number of years it will take for this population to double at an annual growth rate of r), or for the value of r (the annual growth rate within a given time in years, t). For example, if we observe a 3 percent growth rate (r = 0.03), then

$$\frac{\ln 2}{0.03} = t \qquad \text{or} \qquad \frac{0.6931472}{0.03} = 23.1049$$

That is, a population will double (Nt/No =2) in size at an annual growth rate (r) of 3 percent in slightly over 23 years.

Alternatively, if we wish to know the growth rate (r) represented by a population that has doubled in size in 34.6574 years, the formula

$$\frac{\ln 2}{t} = r \qquad \text{or} \qquad \frac{0.6931472}{34.6574} = 0.02$$

indicates that the growth rate (r) is 2% annually.

Suppose, however, that a given population had increased from 20,000 (No value) to 46,000 (Nt value) in 104 years (t). Then Nt/No = 2.3, and the equation

$$\frac{\ln 2.3}{t} = r \qquad \text{or} \qquad \frac{0.8329091}{104} = 0.008$$

indicates that this population grew at an annual rate of 0.008 or 0.8 percent per year over those 104 years. This represents a doubling time in years which can be calculated as

$$\frac{\ln 2}{r} = t \qquad \text{or} \qquad \frac{0.6931472}{0.008} = 86.6434 \text{ years.}$$

Returning to Colin Clark's last observation, we can apply the world growth rate (r) value of about 0.009 realized during the first 50 years of this century to an initial population number (No) of two people, growing to a population of 4 billion (Nt) in 1976 (the approximate world population size at the time Colin Clark made his statement) as follows: Nt/No = 2,000,000,000. ln 2,000,000,000 = 21.416413 and,

$$\frac{21.416413}{0.009} = 2379.6 \text{ years, or about "24 centuries."}$$

The same increase in population (from two individuals to 4 billion) at an annual growth rate (r) of 0.001 or 0.1% would take a little over 21 thousand years, representing a population which doubled its size approximately every 693 years.

Utilizing various statistical estimates of world population size (even the best of today's world population size values are based on such estimates, since actual world census values are not available) based on historical records—together with the formulae presented above—demographers have calculated the size of the world's population 10,000 years ago, just before the advent of food production. Estimates of world population size 10,000 years ago range from 5,000,000 to 10,000,000 (Clark 1977:63). Ehrlich and Ehrlich (1970:5) cite a figure of 5 million; Weiss (1984:642) employs a figure of 6 million; Lee and DeVore (1968:ii) show a figure of 10 million; while Coale (1974:43) and Weller and Bouvier (1981:25) utilize the median value of all estimates, placing it at 8 million.

The date of 8,000 B.C. (or 10,000 years ago) becomes a convenient time marker for further calculations. Whereas Holocene world populations represent a changing mixture of hunting and gathering as opposed to food-producing societies, in the period prior to 8,000 B.C. all the world's populations subsisted exclusively from various combinations of hunting, fishing, and gathering. Thus, world population growth prior to 8,000 B.C. represents world average rates of growth for hunters and gatherers, and the date of 8,000 B.C. together with the estimated world population total of 5 to 10 million people (whichever value we wish to choose) become the Nt values for further calculations.

No values are much more difficult to project or defend. Choosing a convenient and defensible starting point in time requires a series of explicit assumptions based on archaeological and fossil data. Few authors attempt this exercise; and those who do tend (not unexpectedly) to be somewhat vague.

Ehrlich and Ehrlich (1970:9) state that "the populations of our ancestors one or two million years ago (Australopithecus and relatives) were confined to Africa and numbered perhaps 125,000 individuals." Taxonomic issues notwithstanding, Weiss (1984:641) states:

> The genus Homo is thought to extend back at least to 4.5 million years ago [sic], at which time the divergence between hominids and hominoids seems to have taken place. To be conservative, and since the available evidence is somewhat ambiguous as to when the first excursion out of Africa took place, we can set the original species size at 100,000 and let it grow gradually to 500,000 over a period of 3.5 million years. Thus, we estimate the starting species of the genus Homo at roughly 100,000.

I have chosen an admittedly arbitrary time by assuming that members of our genus had reached population densities and developed cultural mechanisms which permitted them to leave Africa around 1,000,000 years ago. Based on the previously cited estimates, this figure would place the worldwide range of No values 1 million years ago between 125,000 and 500,000.

Using these estimated ranges of values for No and Nt, it becomes possible to show that the greatest proportional population change (Nt/No) values are achieved by assuming that No = 125,000, while Nt = 10,000,000. If this increase in world population was achieved in 990,000 years, then the world population yearly growth rate (r) was 0.000004426 or 0.0004426%. This result equals a population doubling time of 156,597.8 years. Utilizing an No value of 500,000 and an Nt value of 5 million, we obtain an r value of 0.000002326 and a world population doubling time of 298,019.7 years!

Even if we were to attempt to increase artificially the yearly population growth rate (r) values, and decrease the required doubling time by adopting the maximum Nt value of 10,000,000 and using Colin Clark's fanciful technique of setting the No value at 2, these figures still would not approach anything similar to the values which have characterized the last 10,000 years. Using these figures r becomes 0.000015581, giving a world population doubling time of 44,487.391 years.

Despite the use of estimated values pertaining to the entire population of the whole world, the significance of these figures should not be misunderstood. These figures set absolute collective limits on average population growth rates of hunters and gatherers worldwide over much of human prehistory. Undoubtedly population densities and growth rates would have differed in different areas, and at different times. Indeed, it would be a mistake to equate these values with the highest (or lowest) growth rates attainable by individual or regional populations of hunters and gatherers over relatively shorter periods of time spanning a few generations or even a few hundred years. I certainly do not propose to use these data in this manner. However, if we were to assume major, sustained increases in these rates over large areas and over extended periods of time, the resultant increases in total population numbers (even though relatively modest during such hypothetical periods of accelerated population growth) would result in an ultimate and major disbalance of numbers by 8,000 B.C.

If these increases in population were to be sustained until 8,000 B.C., the resultant anomalous local increases in hunter and gatherer population densities would have to be justified and documented before such a proposition could be accepted. Alternatively, if after a period of increased population growth rate the resultant increased population densities could not continue to be sustained, we would have to posit some type of (fairly rapid if not catastrophic) population decline to offset the gain in numbers realized in the preceding period of rapid growth.

One of the less obvious corollaries to these requirements relates to the timing of such a hypothetical increase in population growth rates. In a range of time from zero to time point t, the earlier such a sustained rise in population growth rates occurs, the greater its eventual impact will be in altering Nt population values. Increases in population growth rates which occur closer or at the end of this time period will have lower cumulative effects. Consequently, earlier postulated rises in the rate of population growth will incur (if sustained) a much more drastic need to reduce overall population numbers if the resultant increases followed by decreases in growth rate are to equal the calculated population numbers at Nt.

Each of these mandatory alternatives following regional and prolonged periods of increased rates of population growth has fundamental theoretical consequences. If a regional population of hunters and gatherers significantly increases the rate of population growth for an extended period of time, we might be curious as to the cause of this change. Was the change the result of external ecological factors, or was it the result of a signifiant shift in cultural approaches to subsistence? Bender (1975:116-120) seems to suggest that these last two factors form a mutually interdependent feedback loop. In either event, such causal shifts immediately suggest possible lines of archaeological or paleoenvironmental investigation which might be used to test any proposed mechanism responsible for the shift in the growth rate of the population.

Allied to this set of assumptions we may ask some other questions. If the population stopped growing, or if the population declined or crashed in numbers following such a period of accelerated growth rates, what brought on this new set of demographic changes? Furthermore, what evidence can we find to suggest that major declines in population took place? Once again, various empirical tests could be devised to test hypotheses aimed at answering such questions.

One researcher (Smith 1972:15) seems to propose an answer to the second question stating that he suspects that "...many cases of so-called cultural devolution may in fact be the consequences of severe declines in population and the disappearance or reduction of demographic pressure." Applied to small-scale societies within the terms and concepts discussed earlier, Smith seems to suggest that within stable environmental parameters where human population numbers decrease, there would be a decrease in competition for resources resulting in a decrease in systemic foraging and a proportional increase in individual foraging strategies and technologies. Unfortunately, however reasonable such a proposition may be, it does not address causal issues for demographic decline. Furthermore, given world demographic values around 8,000 B.C., we must still conclude that episodes of demographic decline in the past must have been short-lived and localized phenomena.

In a table accompanying the previously cited population estimates, Weiss (1984:642) presents a series of additional estimates which we may evaluate in light of the issues which I have just raised. Weiss (citing other sources) estimates world population figures as 100,000 at 4.5 million years ago; 500,000 at 1 million years ago; between 500,000 to 50,000 years ago the population remained unchanged at 1,300,000; and 6,000,000 people at 10,000 years ago.

Calculating the rate of world population growth (r) between these intervals, we find that the rate of growth between 4.5 million and 1 million years ago equals 0.0000005; between 1 million and 500,000 years ago r equaled 0.0000019; between 500,000 and 50,000 r equaled zero (i.e., no growth whatsoever); and between 50,000 and 10,000 years ago r would have equaled 0.0000382. The overall growth rate from 1 million to 10,000 years ago would have shown an r value of 0.0000025. In light of these figures it is interesting to note that elsewhere Ward and Weiss (1976:5-6) dismiss the view that "...r has rarely risen above 0.1% in man's history..." as nothing more than a "contentious opinion."

To be fair in evaluating this last conclusion, it must be pointed out that recorded history, does document frequent examples of population growth rates in excess of 0.1% (that is, r = 0.001) among food producers and complex civilizations. Furthermore, Ward and Weiss (1976) do not give any further parameters as to the size of the population or the length of time during which r values of 0.001 might have been exceeded. However, the context of their original statements appear to be oriented toward calculating demographic factors which affect relatively small, localized populations for very short periods of time (at most a few generations), rather than sustained, large-scale, and long-standing shifts in growth rates of substantially large segments of the world's population.

The view that localized populations in prehistory may have fluctuated in their size and growth rates without yielding a significant net increase in world population or affecting the rate of world population growth is echoed by others, such as Hammel and Howell (1987:142), who state that "the zero or near zero net growth of global population could have been an average of widely differing local population growth rates." This view is in accord with the interpretation of the perspectives of Ward and Weiss (1976) discussed above. Yet neither set of authors seems to suggest radical departures from the extremely slow world average population growth rates in prehistory either by larger population aggregates or over extended periods of time.

The reason why I have pointed out these limiting factors to growth rates approaching or exceeding r values of 0.001 can be understood better if we intentionally violate these limits in a hypothetical example. To obtain an initial arbitrary scale we may note that in the mid 1960s about one fifth of the world's population lived in Europe, while the land surface of South America represents roughly one eighth of the land surface of the earth. If we were to assume, then, that some combination of various (local and regional) populations equivalent to one eighth of the world population estimated by Weiss (1984) for 50,000 years ago had undergone a sustained population growth rate increase equal to (r) 0.001 or 0.1% per year, the resultant progeny of this original subset of the estimated world population total of 1,300,000 (or about 162,500 people) would have produced an unprecedented growth in human numbers over a relatively short interval of time.

In 3,610 years (or by 46,390 years ago) the progeny of this subset of the world's population would have exceeded the 6 million total world population figure estimated by Weiss (1984) for 10,000 years ago; by 40,800 years ago the population would have grown in excess of the 1.95 billion world population value for A.D. 1900; and by 39,500 years ago it would have outstripped the current (A.D. 1990) total world population. Clearly, then, if population growth rates in prehistory exceeded r values of 0.001, such events must have been very localized and extremely short-lived phenomena.

In keeping with these conclusions it is interesting to note that in a discussion of paleodemographic growth patterns in the Old World, Ammerman et al. reach the following two conclusions:

> (1) it is unreasonable to expect the population of an area to continue growing at a rate of 1 to 3 percent per year over a period as long as one thousand years, and (2) even such an apparently low rate of growth as 0.1 percent per year will lead to major changes in population size and probably to socioeconomic changes as well if it is maintained over periods on the order of two thousand years (Ammerman et al. 1976:29).

The significance of all these calculations to an understanding of demographic variables which characterize small-scale societies can be summarized as follows. As is frequently pointed out in the anthropological literature, for more than 99% of the span of human existence people were dependent on a hunting and gathering subsistence base. Minimizing some of the potential complications arising from biological differences found in earlier ancestral populations, we find that during the period between 1 million and 10,000 years ago, members of our own genus (including *Homo erectus* and *Homo sapiens*) showed world population growth rates of about 0.0003% (r = 0.000003) per year. This rate equals a world population doubling time of 231,049 years (assuming a world population of 500,000 people 1 million years ago, and 10 million people by 10,000 years ago). Even if we utilize the figures given by Weiss (1984) for population growth between 50,000 and 10,000 years ago (for *Homo sapiens* exclusively), the growth rate only rises to an r value of 0.0000382, with a doubling time equal to 18,145 years.

Earlier calculations (above) also demonstrate that although variations on this rate of growth may very possibly have taken place in the past, such variations (if involving larger populations or extended periods of sustained growth rate increases) would have quickly produced unrealistically large populations for which there is no empirical support from the archaeological or fossil record. If the world population estimates for 10,000 years ago are to be accepted as accurate, even to the level of a margin of error in excess of several orders of magnitude, then such unrealistically widespread and rapid population increases would have had to result in mandatory vast and rapid (perhaps catastrophic) declines as well.

Consequently, and although still granting that short-lived fluctuations affecting very small local populations would have still been possible and perhaps likely, we must conclude that the perspective which holds that prehistoric small-scale populations generally grew at only vanishingly small rates in prehistory must indeed be correct. No other conclusion can reasonably be supported based on the figures and calculations presented above. The hypothetical growth rate of r = 0.001 is anywhere from 26 to over 300 times greater than the world growth rates calculated from the data provided by Weiss (1984).

In light of these observations, I feel justified in adopting a demographic perspective of small-scale societies which stresses the maintenance of population numbers at, or very nearly at equilibrium values through periods of a few generations or several hundred years. Through such periods population numbers may fluctuate around some average figure which remains relatively stable over long periods of time.

The adoption of such a perspective, in turn, requires that we should explore and address some of the potential mechanisms which could account for the occurrence of demographic stability among small-scale societies. Generally, the approaches which have been employed in the literature involve the use of biological (naturalistic) constructs, or deliberate cultural mechanisms. Often a single mechanism, either an internal cultural mechanism or an external (environmental) stimulus are invoked in the context of a simple cause-and-effect relationship. Caldwell et al. (1987:30) who have reviewed past uses and misuses of demographic data by anthropologists conclude that there is

> ...no need to search for large-scale deliberate fertility control to explain lack of growth in human numbers. It is

> probable that the mortality extremes of primitive populations lie between expectations of life at birth of 20 and 30 years. In the equilibrium situation the former would be consonant with crude death and birth rates around 55 per thousand and an average of 7 births per woman, while the latter would be in keeping with death and birth rates nearer 35 and just under 5 births per woman... In both societies the mortality level might well still be controlled by the availability of food, especially in times of shortage. Certainly, in the latter, life would appear to be easier in that deaths were fewer, but the main impact would be on infants and children, as is evidenced by the fact that, while the expectation of life differed between the two societies by 10 years at birth, the gap was only about 5 years at 20 years of age and 3 years at 40 years of age.

In the next section I shall try to integrate some of the processes and principles discussed earlier, with concepts such as "carrying capacity" and specific biocultural observations on the dynamics of small-scale societies which directly support the projection of natural demographic equilibrium advanced by Caldwell et al. (1987).

3.5 SMALL-SCALE SOCIETIES: DYNAMICS OF POPULATION STASIS AND GROWTH

One "naturalistic" concept which is frequently invoked in relation to limiting factors in human populations is the concept of carrying capacity. At times, carrying capacity has been used as a fundamental and active dimension limiting the size of human populations (cf. Zubrow 1975). Although valid arguments can be developed to indicate that the finite resources of any given environment can act to limit population size, one should not automatically assume that a simple relationship exists between resources and the size of human populations.

Hayden (1981:352) has pointed out that "...despite the widespread use of the concept of carrying capacity, it is operationally unfeasible to determine what this balance might be for any given region or human group." It may be argued that such a statement does violence to the use of the concept from its strict definition in the context of biological ecology, in which carrying capacity is usually an inductively observed value. In one sense, carrying capacity, as defined in biological ecology, stands for the observed population numbers (density) of a particular species in a specific habitat.

Observed population numbers are sometimes expressed in graphic form as population growth curves. Exponential growth (such as the one characteristic of our own species during this century) produces a "J-shaped" growth curve; while, a simplified depiction of the most commonly encountered growth curve among natural populations leading to equilibrium or "K-selection" is the "S-shaped" or sigmoid (logistic) growth curve (see Odum 1971:184, 257; also, see Figure 3.2, below). The population growth shown in Zone "A" of the sigmoid growth curve (Figure 3.2) gradually slows in response to density-dependent negative feedback effects (Zone "B") until equilibrium is established at the "upper asymptote" (the upper right end value of the curve).

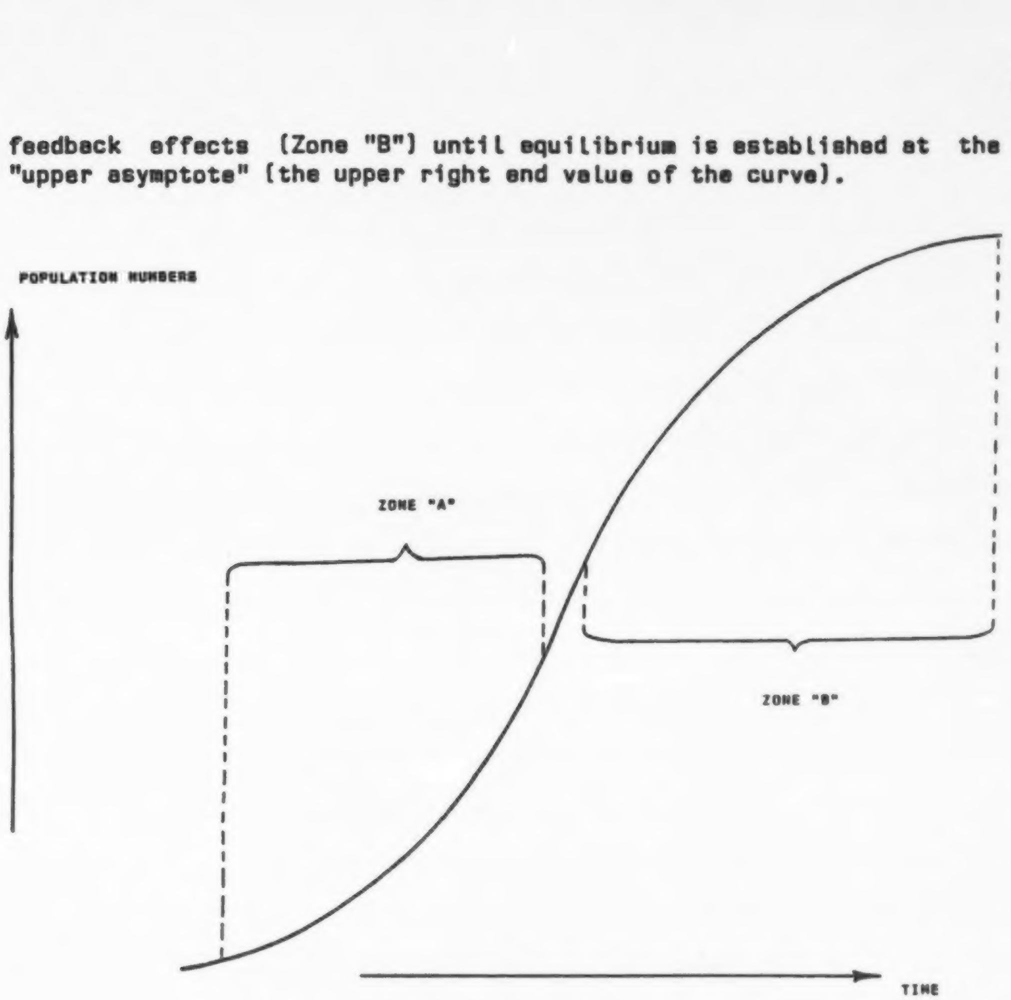

Figure 3.2
Simplified Sigmoid Growth Curve

Although we normally think of carrying capacity as some type of relatively stable set of numbers, this need not be the case. Carrying capacity will vary over time. This factor becomes clear when we consider the various components of the formulas utilized in biological ecology to model carrying capacity mathematically.

Mathematical models of carrying capacity used by biological ecologists incorporate several critical components which include resource availability (including the rate of resource replacement for renewable resources), the innate reproductive capacity of organisms, the level of individual resource requirement for survival, and the overall population size at a given point in time (Ricklefs 1979:560-566). It is the interactions of all of these factors which yields the observed population numbers or density for a particular population, in a particular habitat, at a specific point in time. Stable population to resource ratios (that which is often implied by some authors using the term "carrying capacity"), are the result of temporary dynamic equilibria between these various factors.

Furthermore, resources, either as environmentally available, or as resource requirements of particular organisms, must be defined very broadly. Not only do "resources" in this context include caloric energy, protein, specific nutrients, or water, but they also include shelter, living space, and other niche axes essential to the survival of the individuals which form part of specific populations. To these the anthropologist, studying human beings who adapt through specific cultural systems, must additionally include not only the innate biological requirements listed above, but also specific requirements which arise partly or entirely as a result of the specific systemic attributes and requirements of the particular cultural adaptations employed by a specific human population at any particular point in time. This last requirement validates Hayden's observation (1981:352) that it is impossible to define operationally the specific form which the balance of resources to human population numbers will yield in the context of specific situations.

The problem implicit in Hayden's statement and in the typical approach to the definition of carrying capacity which frequently appears in the literature is precisely the failure to acknowledge the extremely complex and dynamically variable nature of what is meant by carrying capacity. Michael Glassow states that "in defining carrying capacity, it is first necessary to realize that the term has been used in two quite different senses: as an abstract concept in theoretical constructions, and as a measure of specific aspects of the relationship between a particular human population and its environment" (Glassow 1978:32). To these I would add a third (and I would argue, unrealistic) use of the concept—namely as a means of predicting the potential size of human populations as projected from relatively simple tallies of available environmental resources.

Viewing carrying capacity as a "dynamic equilibrium system," Zubrow (1975:15) defines carrying capacity as "...the maximum size of a population which can be maintained indefinitely within an area." Needless to say, when speaking of small-scale societies one immediately encounters difficulties related to defining the "area" (territory?) involved, and in defining and tabulating what might be considered culturally accessible or relevant resources within the "area" (cf. Bender 1975, and Smith 1972). Beyond these concerns, Zubrow's definition also corresponds to one of the possible uses of the concept in biological ecology, in which this "abstract concept" can be used in "theoretical constructions" referring to "...the upper asymptote of a sigmoid curve—a particular characteristic of a graphical model" (Glassow (1978:32). Unfortunately, this same definition, by its very simplicity, hides the multitude of specific factors which must be operationally quantified before these relationships can yield concrete and reliable numerical predictions. Examples and discussions of some of the inherent difficulties associated with the carrying concept and its application to the modeling of small-scale societies can be frequently found in the literature (cf. Glassow 1978, Weisberg 1975, Black-Rogers 1986, Hayden 1981).

The perception (on our part) that hunting and gathering populations exist in some stable equilibrium number somehow below the expected carrying capacity "of the environment" should simply serve as a warning to us that our conception of what constitutes carrying capacity, or how we project our expectations of carrying capacity values on to specific instances is in need of revision. In a sense, this mistaken perception may reflect the inherent difficulties associated with operationalizing a difficult and complex concept.

The multitude of component factors and their interrelationships, which yield various carrying capacity levels for various populations in specific habitats, can not be simply boiled down to some universal and simplistic set of relationships. When applied to culture-bearing human populations (as I have noted earlier), these relationships become increasingly complex by the addition of cultural variables to the natural variables listed earlier. This factor, more than anything else, prevents us from "operationalizing" the carrying capacity concept successfully so as to yield testable predictions. As one author has put it,

> ...'carrying capacity' is not the maximum number of individuals a given _area_ will sustain, but rather the maximum number of individuals a given _sociocultural system_ can sustain in a given area, depending on its mode of production, its technological level, and its cognitions concerning proper levels of density, etc. 'Extractive capacity' might be a better term for this concept (Bargatzky 1984:404) [emphasis in original].

At the same time, a distinction should be made between the difficulty (if not the outright impossibility) associated with attempts at developing workable predictions, and the theoretical assessment of component relationships which influence the empirically observed carrying capacities of populations. Failure to generate reliable predictions should not be equated with a requirement that consideration of the component relations of the carrying capacity concept should be neglected or avoided.

Hayden (1972) rejects the effectiveness of innate physiological mechanisms in controlling human population growth rates. However, he proposes a set of mechanisms based on a variety of well-documented reports which point to aggression, division of labor, abortion, infanticide, and social group dynamics within the context of environmental parameters which interact with each other to provide effective population control. In effect, Hayden (1972) proposes that human population numbers are prevented from reaching critical resource shortage levels through the action and intervention of a variety of cultural mechanisms.

Without intending to challenge the validity of the mechanisms proposed by Hayden as active components in a process which serves to limit human populations, I would like to call attention to a large and growing body of literature which indicates that human cultural behaviors coupled with physiological factors, can, and very likely do serve to limit population size in small-scale societies. Lee (1979), Cashdan (1985), Kolata (1975), Roth (1981), Frisch (1972, 1975, 1988) and others have consistently pointed out a variety of physiological factors which (interacting with the cultural realities of small-scale societies) result in mechanisms which effectively control the size and growth rates of human populations. As Jane Lancaster puts it,

> In most nonhuman primates adult females give birth every one to four years (depending on the species) from sexual maturity until death. Although a female monkey or ape can easily produce 10 to 15 offspring during a lifetime, most of these will die before sexual maturity. In fact, a number of field studies have reported 50 percent infant mortality during the first year of life. Among humans, too, the death rate of infants has been high in time past due to factors such as infectious disease and parasites. The high death rate of infants, combined with a birth spacing of two or three years due to low fertility during lactation, meant that in time past most human populations were relatively stable or only expanding slowly (Lancaster 1975:84-85).

Population size and growth rate comparisons between hunters and gatherers while actively mobile and subsequently upon adoption of more sedentary habits have been reported by Roth (1981), Howell (1976), Kolata (1975), and others. Kolata (1975:101) notes that "the population growth among the sedentary !Kung results from both a decrease in the age of menarche and a decrease in the average time between births." Both of these physiological shifts are related, in turn, to an increase in the body fat of women (cf. Frisch 1988). The shorter birth spacing is further augmented by the availability of milk and grains among the !Kung. These foods make it possible to wean children at an earlier age.

Eric Roth (1981) notes for the Kutchin Athapascans that the population increase realized by the sedentary Kutchin results from decreased infant mortality in the first five years of life, and from women's "...increased utilization of the[ir] total potential reproductive period" (Roth 1981:421). Roth ascribes these effects to the impact which increased body fat has upon women's reproductive capacity.

Cashdan (1985) focuses on increasing birth spacing length in relation to its effect on limiting population growth. Although Cashdan explores the role played by various cultural mechanisms such as abortion, abstinence, and infanticide in increasing birth spacing, it is clear that increased birth spacing due to naturally occurring high infant mortality will also result in increases in birth spacing (cf. Roth 1981:420-421). Shorter birth spacing may well result in a variety of pressures on reproductive women. Cashdan notes that

> The economic rationality of long interbirth intervals among foragers has been recognized by many researchers, who argue that such spacing is necessary because of the difficulty of transporting two nursing infants plus a load of gathered foods (Cashdan 1985:652).

Indeed, as Lee puts it,

> The actual work involved in raising young children in a

> hunting and gathering society is in large part a function of three variables: weight of children, the distance to be traveled, and the frequency with which children are born to a given woman (Lee 1979:312).

As the work requirement placed on reproductive women increases, their physiological capacity to divert energy toward reproductive functions decreases. Less energy will be stored as body fat, the approximately 1,000 additional calories required by lactating women will be harder to meet, and the incidence of amenorrhoea will increase, resulting in a decline in the rate of conception while increasing the average birth interval. Clearly, then, the strict cultural division of labor which allocates gathering activities along with the bulk of the labor costs of caring for infants (in addition to the physiologically derived energy costs) to reproductive women, results in a combination of factors which can severely limit the reproductive capacity of a small-scale society.

In the above stated context, infant mortality (whether natural or due to intentional infanticide) results in a variety of added reproductive "costs" which can directly contribute to the maintenance of relatively stable population numbers. In addition to the increase in the average birth spacing length associated with infant mortality, the physiological energy costs of pregnancy (and any investments incurred through subsequent lactation) represent investments of energy from the total energy budget of a small scale society which are lost with the death of the infant.

The full impact of this loss should be evaluated on the basis of the mechanism of energy transfer and the effects of the second law of thermodynamics as demonstrated in biological ecology. The biomass (expressed in some total caloric value) represented by an infant in the absence of these other considerations appears, at first, to be almost negligible. However, we must realize that the bodily energy of the infant represents what ecologists call its net productivity.

Net productivity is the energy (in the form of organic compounds and tissues or biomass) which is left after the costs or expenditures of respiration (basal metabolic costs, muscle movements, and other physiological functions) have been subtracted from its gross productivity (its total energy gains). A lactating infant's gross productivity is derived from the mother's net productivity. The mother, in turn, incurs losses due to respiration. Since the efficiency of energy transfers is always some value below 100%, and in the case of biological energy transfers is often under ten percent, each mandatory transfer of energy in this sequence results in considerable losses of energy. Put simply, the net productivity represented by the biomass of a fetus or a lactating infant represent a disproportionately large parental gross energy investment on the scale of several orders of magnitude.

Our own modern cultural perspective, based as it is on the availability of unimaginably vast quantities of energy supplied by fossil fuels and other extrinsic sources, constitutes a poor source of "intuitive" insight into the energy dynamics of a small scale society operating in the context of a very clearly limited and finite energy pool. In the previously cited comparisons of birth rate increases for the sedentary !Kung and Kutchin (as compared to birth rates for their more mobile predecessors) a major factor appears to have been the increased availability of food energy and concomitant increase in the bodily fat of reproductive women.

Although the source of this additional food energy to women might be interpreted as the result of changes in the diet, the same results could be obtained without significant dietary changes if the workload performed by women were to be reduced. Lee's observation of the energy costs incurred by women due to gathering and child transport activities is particularly significant in this context. Under sedentary conditions, both of these requirements are relaxed, freeing energy which can then be invested in reproduction. The energy expended on movement by mobile, hunting and gathering societies represents a major energy liability to this mode of existence. Baker (1978:119) cites a comparison of energy costs of movement in mobile and sedentary groups, pointing out that the percent of energy expenditures devoted to movement shift from 50 to 60 percent or more in hunting and gathering societies to only half that value in agricultural societies.

Altogether, the interaction of all of the factors which impinge on the energy investment represented by children to small-scale societies presents staggering implications. Some of these factors and their implications may be summarized as follows:

1) The absolute abundance of prey species (both plants and animals) is an indirect, rather than a direct measure of the carrying capacity of a habitat. The actual carrying capacity is a product of a variety of component factors. Among these factors we must consider the prioritization of prey species, and the effects of the marginal value theorem, which predict that hunters and gatherers will always harvest prey only selectively, and then also, only a fraction of the total number of available prey in the habitat. As I have tried to indicate, this pattern may appear to suggest some form of conscious conservation practices on the part of hunters and gatherers—a conclusion which is not automatically warranted in the absence of corroborative empirical evidence in individual instances.

All of this simply means that despite large quantities of available prey (and therefore potentially high food energy availability to hunters and gatherers), most hunting and gathering populations, in actual practice, will be able to only use a fraction of the total productivity present in their habitats. In other words, the energy budget of a small-scale society should be measured by the "bird actually in the hand" rather than by the "hundred in the bush."

2) The biological reproductive role of women, coupled with the labor costs and energy liabilities incurred through mobility, and the work associated with gathering food and fuel, and transporting infants (due to strict division of labor), in combination place an enormous demand on the energy budget of reproductive women. This set of circumstances sets reproductive women aside as a separate category in the assessment of the energy budgets of small-scale societies.

Reproductive women, who after all can only access a portion of the total energy budget of a small-scale society, must meet from these energy receipts some disproportionately enormous and mandatory energy expenditures. This is the true context of the "availability" of energy which a small-scale society can invest in the production of new members by reproductive means. The remainder of the food energy available to other members of a small-scale society should not be included in the pool of energy available to insure the reproductive success of the population.

3) Finally, the energy represented by the biomass of infants should be viewed in the context of the disproportionately large amount of the total gross energy available to reproductive women which goes into the production of each unit value of the biomass of infants.

When all three of these considerations are incorporated into an assessment of the capacities of small-scale societies to invest energy into the overall reproductive strategy of the population and its capacity to increase significantly, let alone maintain population numbers, a different perspective emerges. From this perspective, it is truly amazing that small-scale societies are able to maintain their population numbers anywhere near an equilibrium level, let alone to increase their numbers. Also, we can better appreciate the tremendous impact which infant mortality has on the energy allocated by a small-scale society toward insuring its reproductive success. Whether infant mortality results from natural or deliberate processes, the loss of the maternal energy investment represented by infant mortality (when averaged together with the energy costs associated with the production of surviving infants) produces a net increase in the total energy investment represented by each surviving offspring—placing further demands on the total food energy base available to reproductive women.

Since the total food energy base available to reproductive women is clearly finite, one can theoretically conceive that an ultimate point may eventually be reached where such energy constraints can act to limit directly the reproductive rate and success of a small-scale society. Within this context, infant mortality, more than any other demographic process, seems to be responsible for providing the final component which helps to maintain the relatively stable population values associated with small-scale hunting and gathering societies. In view of this conclusion, it is interesting to note that major increases in the rate of population growth of "developing nations" during this century have invariably been associated with a drop in infant mortality (cf. Bogue 1969; Clark 1977; and Weller and Bouvier 1981); while a similar pattern is documented and highlighted for the recently sedentary Kutchin Athapascans by Roth (1981).

Whereas changes in the rate of infant mortality in modern examples may well be the result of improvements in sanitation and medical care, the factors influencing the rates of infant mortality for small-scale societies in prehistory are not so easily explained. Cultural practices involving abstinence, abortion, and infanticide undoubtedly played a significant role. Unfortunately such practices may have varied greatly, and their very existence in these contexts is extremely difficult if not impossible to document.

There is, however, one more mechanism which we might consider in relation to infant mortality in small-scale societies. This mechanism, although based on cultural practices associated with hunting and gathering subsistence patterns, is much more likely to be present in a wide variety of societies, since it depends on the very mode of subsistence, rather than on unique or stochastic traits associated with individual cultures.

In an earlier segment of this chapter devoted to a discussion of the foraging strategies employed by small-scale societies, I contrasted the contribution to the overall diet made by individual as opposed to systemic forms of foraging. At this point I would like to suggest that the relative contribution of each of these types of foraging differs significantly for small-scale society members based on age and sex. The differences which I propose are based not only on the hunting as opposed to gathering specialization of men and women, but also on the relative differences in mobility of each of these groups. Except in cases of group relocation, men typically travel greater distances, and therefore utilize a larger area for individual foraging than do women and children. The differences in the foraging areas utilized respectively by each of these two groups differs significantly in other respects.

Mobile hunters and gatherers employ foraging strategies which not only involve the use of a central place, but the local group may frequently move over the course of a year, occupying a succession of various such central places. Although the timing and destination of each of these moves involves an assessment of a number of important factors, clearly one component obtains from the gradual depletion of prey density around the central place, resulting in increased search and pursuit energy costs (see Binford 1978:458 for a discussion of logistical mobility). Thus the history of occupation of any one such site over some portion of a year can be viewed as having several sequential stages.

In the first stage, the group arrives and settles in, constructing shelters and surveying the immediate area to update its knowledge of the location and availability of critical resources. In the next stage, all members of the group derive the maximum benefit from both individual and systemic foraging efforts. As time progresses, however, the presence of foragers engaged in active harvesting of the available prey will result in an unequal pattern of resource depletion. Those areas located nearest the central place will have been most frequently visited by individual foragers, and therefore these areas will have been the first areas to have become depleted of prey. "Silberbauer notes that plant supplies begin to dwindle and game animals learn to avoid hunting zones within about four weeks" (Hayden 1981:375). As this process continues over time, individual foragers will be forced to extend their foraging efforts to ever-more distant locales, away from the central place.

This gradual and uneven depletion of suitable prey over space will result in a differential impact upon men as opposed to women and children. Even if systemic foraging is left unaffected, the proportion of food available through individual foraging to men as opposed to women and children compared to the amount of food available from systemic foraging will gradually shift in proportion to the degree of depletion of localized prey densities.

Men, whose foraging range extends beyond the range of women and children, will continue to meet with success in their efforts at obtaining food through individual foraging. At the same time, the depletion of local resources nearer to the central place and within the foraging range of women and children will steadily decline. Women may still be providing the bulk of the systemic food consumed by the group; however, the variety of food types encountered will become increasingly limited. Furthermore, the quantity of naturally occurring food types capable of direct consumption in the field, not requiring processing before consumption in the central place, will also decrease.

The net result of these changes will be that while the absolute volume of available food may be unaffected for some time, the nutritional value of a diversified diet will continue to yield its benefits to foraging men, while foraging women and children will soon be restricted to a more monotonous diet. In the case of children there are added associated consequences.

Children typically have a faster metabolism than adults, while at the same time they lack the bodily reserves of adults. These facts mean that children require more regular and (by body weight) disproportionately more energy and nutrients than adults. If these needs of children traveling with their mothers are to be met, women will also have to carry provisions of food and water while out gathering. This food must be obtained either by individual foraging, on the spot; or if such food is unavailable, then it must be carried from the central place—further drawing on energy obtained through systemic foraging.

In all likelihood, most of the favorable habitats and most of the yearly cycle of foraging by small-scale societies would have permitted a successful resolution of the energy and nutritional problems presented by this pattern. However, in marginal environments, or during moderate to severe scarcity, these mechanisms probably resulted in severe nutritional stresses on women, and particularly on children—even in the face of food sharing which might have kept them from starvation in the literal sense.

The nutritional deficiencies which would accompany the pattern I have described would undoubtedly have led to a lowering of the body's capacity to resist disease and parasites, while impacting on women's reproductive and lactating capacities. Nutritional deficiencies could cause reduced fertility and increase infant mortality even when the group's health and food supply do not appear threatened.

It should be noted that even in the absence of seasonal periods of shortage, these effects would ensue for all mobile hunters and gatherers due to the intrinsic mechanisms governing central place location; relocation; differential foraging ranges; and the differential contribution of individual and systemic foraging to the diets of men, women, and children in small-scale societies. The effects described would tend to be very subtle, and generally unlikely to result in overt mechanisms aimed at compensating for these problems. Thus, this pattern is likely to have occurred widely among hunters and gatherers everywhere, both past and present.

Whether one could argue from this vantage point that such a mechanism would have sufficed to account for infertility or infant mortality operating at rates sufficiently high to balance the natural tendency of human populations to increase in numbers is altogether a different question. In theory, the recognition of these effects, should not be equated with a demonstration or quantification of the direct results of this pattern of population control. Nevertheless, given the demonstrated tendency toward demographic equilibrium encountered among small-scale societies over wide ranges of habitats and over long periods in prehistory, it seems that we should seek to discover some type of population control mechanism with wide spatial and temporal applicability, such as the one I have just outlined above, rather than particularistic and potentially variable explanations which have no basis by which they could be generalized to all or most of the known societies of hunters and gatherers.

3.6 SMALL-SCALE SOCIETIES: GROUPING AND DISPERSAL TENDENCIES

At the very minimum, any discussion of grouping and dispersal tendencies should begin with a recognition that such tendencies can be viewed either from an individual's or a group's perspective. In other words, the overall topic entails a discussion of the population density of individuals, on the one hand, and the frequency (or territorial spacing) in a larger area of discrete groups. Perhaps because human beings are viewed as adapting through social, rather than solitary means, the distinction between individual and group dispersal is not always clear in the anthropological literature—particularly in situations in which ecological analogues based on non-human species are used.

Among non-human organisms, the dispersal of single organisms or pairs of organisms which maintain a territory is tied to the availability of critical resources; and is often expressed as the relative population density of that organism within a given area. This concept should not be confused with the concept of dispersion, which addresses the tendency (by age, sex, or other factors) of individual organisms to leave the territory of their birth and to establish themselves in a new location.

Although geographical territories and population densities are important because living space itself can constitute a limiting resource, the use of this measure is more frequently due to the practical need for expressing the relative dispersal of a species by means of some standardized, convenient, and comparable unit of measure. In fact, most ecological discussions (as well as many anthropological discussions) of dispersal expressed as numbers of individuals per unit of area appear to be aimed at some measure of the relationships between the population density of a particular species relative to the density of some critical resource or resources (such as prey species or water) within that same area. Such considerations frequently constitute the focus of discussions of the theoretical "carrying capacity" of a habitat—a concept which I have discussed earlier.

Variation in resource availability has also been used along with mate selection and avoidance of inbreeding to explain the reasons for dispersion behaviors observed in nature. Wynne-Edwards (1962), and more recently Baker (1978) have provided detailed comparative zoological treatments of animal dispersion patterns which have also addressed the question of human dispersion patterns.

Perhaps the most consistent factors influencing the non-random composition of emigrants in individual species appear to be dominance, and the relatively high investment of habitat-specific familiarity, by reproductive individuals with offspring compared to emigrants. These factors often express themselves through the non-random sex and age composition of emigrants.

> In some species, dominance, or order of precedence within the group, is a major organizing principle. Dominance hierarchies are the outcome of successive challenges and confrontations over rights to physical space and access to sexual partners. However, given a concentration of food in one place (as may occur in times of shortage), the same hierarchy is available to regulate feeding rights. Although dominant animals feed to satiety before allowing others to begin, habituation to the established hierarchy is usually sufficient to curtail fighting. The behavior involved in respecting a dominance hierarchy is consistent with the principle of individual maximization of benefits (Abernethy 1979:20-21).

Although differences in sex are frequently more variable relative to selective factors influencing emigration in various species, mammalian patterns of emigration often favor a disproportionately high frequency of emigration by subadults or young adults. Given the existence of dominance and a social hierarchy, in situations in which population increase or dwindling resources result in increased levels of stress, the individuals forced to leave are usually "the junior fraction of the hierarchy" (Wynne-Edwards 1962:483). Lidicker (1975:109) notes:

> The literature on small mammals is rich with references to dispersal dominated by juveniles. Certainly this is a common mammalian pattern, and may produce breeding populations with fewer young than would be expected on the basis of recruitment rates. Correspondingly, new colonies or low density groups may be more youthful than expected.

The relative youth of dispersers is supported in the biological literature not only for small organisms such as prairie dogs (Halpin 1987), but also for larger mammals such as wolves (Mech 1987) and black bears (Rogers 1987). Undoubtedly, decreased resource availability increases search and capture time, decreasing foraging efficiency. Emigration can act as a "safety valve" in situations in which a population has saturated its habitat, offering benefits to both those who leave, and those who remain behind. Furthermore, it is clear that a social hierarchy can actively encourage lower ranking individuals (usually prereproductive individuals) to leave. However, none of these observations explains why the younger individuals are more likely to leave, or why older individuals are more likely to remain.

As earlier, remaining within one's territory is advantageous as a result of intimate familiarity with topography, resource availability and location, access to shelter, and other such intrinsic advantages concomitant with a standing familiarity with one's territory. Such habitat-specific information will have to be gained anew by emigrants reaching and establishing themselves in new, unfamiliar territory. The availability of a suitable alternative habitat for occupation by emigrants is far from certain. Furthermore, time spent on travel through unfamiliar territory can be costly due to hazards such as predation, and as a result of lost reproductive time. On the other hand, locating a new and suitable habitat can ultimately enhance the reproductive success of individuals, while improving their capacity to achieve a high ranking position in a developing social hierarchy.

> As with dominance, territoriality guarantees rights to a food source, but the fighting among conspecifics claiming an area occurs in the season when there is no food shortage. The immediate individual drive appears to be directed toward space and mates. Thus, territoriality not only limits pressure on food resources by dispersement; it has been observed that birds of the territorial type which do not succeed in defending a territory will not mate, and therefore the territorial drive also functions as a constraint on reproduction [Abernethy 1979:21] [emphasis in original].

Perhaps more subtly, we may point out that in instances such as the one highlighted by Abernethy [above], the initial choice of a territory to be defended against conspecific challengers can be demonstrated to meet certain minimum requirements directly related to the subsistence and safety of a mating pair and their offspring. Often in the case of birds [as cited by Abernethy], a male will lay claim to such a territory, defending it against challengers, while attracting potential mates. Mating and reproduction occurs at the end of this sequence, and it is dependent on the successful conduct of these preceding behaviors.

Although birds which can not successfully proclaim and hold territory may fail to mate and produce offspring, birds occupying marginal territories show that even when mating takes place, if minimum territorial qualities are not met, the number of offspring produced will decrease, while in extreme cases no offspring may survive. In other words, failure to mate may be only one important consequence among several factors which prevent individuals which are unsuccessful in holding territory from producing viable offspring [cf. Wynne-Edwards 1962:145-157].

Reproduction and the raising of the young places on-going and significantly higher nutritional and energy demands upon reproductive adults than on prereproductive individuals. A social hierarchy promotes stability while ensuring that these requirements are met, conferring a selective advantage upon reproductive adults and the population at large. Overall, the best advantage of all these considerations will be realized by populations which engage in what Lidicker has called "pre-saturation emigration."

> Pre-saturation emigration is an exodus from a population before its habitat becomes saturated with that species. That is, it occurs during population growth, and may even begin very soon after growth starts. Moreover, such emigrants (exhibiting pre-saturation dispersal behavior) are not necessarily destitute either socially or economically, as there remains a surplus of resources in their home environment... Compared to saturation emigrants, such individuals can be expected to have a much greater chance of surviving and re-establishing themselves elsewhere (Lidicker 1975:106).

Because pre-saturation emigration favors the emigration of individuals prior to the onset of stress induced by resource scarcity, Lidicker notes that such emigrants will tend to be more representative of a broad cross-section of age and sex of the parental population (ibid.). In pre-saturation dispersion, a different set of causal mechanisms have been proposed from those which influence dispersion upon saturation of a habitat.

> As populations become denser, intra-specific interference may result in inefficient utilization of remaining resources. For example, resources may be scattered where previously they were clumped, the most desired food or shelter may be scarce, more time and energy may be spent in social interactions, or activity periods may be shifted to less optimal times. Consequently, individual fitness may be improved by leaving rather than continuing to utilize available resources inefficiently (Lidicker 1975:107).

Despite these advantages, the unequal demands placed on individuals actively reproducing and raising young, as opposed to adults without these requirements, will still serve to encourage a greater proportion of reproducing adults, and particularly reproducing females, to remain behind. Furthermore, pre-saturation dispersion can only continue to operate successfully as long as there is a habitat nearby known as a "dispersal sink" which can absorb emigrants. For relatively slow-growing, stable, K-selected populations, such a dispersal sink may not exist in nature, except rarely upon entry into a large, heretofore unoccupied land mass.

In view of these factors, it is interesting to note that in applying these processes to human small-scale societies, Baker (1978:120) chooses a model which seems to be founded on saturation (rather than pre-saturation) dispersion. Focusing on the dispersion role of subadults and prereproductive adults, Baker states:

> It is suggested that the migrations of human hunter-gatherers up to the early reproductive phase of ontogeny should show the following components:
> 1 extension of the familiar area by sub-adult males and

> sub-adult females;
> 2 greater familiar area extension by sub-adult males than by sub-adult females;
> 3 familiar area extension by migration by sub-adults based largely on socially communicated information;
> 4 less correlation between the areas visited by sub-adults during familiar area extension and food availability and deme density than for any other ontogenetic class, though stay-time may be longer in areas of high deme density;
> 5 less territorial resistance to immigrants when food is abundant than when food is scarce;
> 6 removal migration of a pair some time after formation of a permanent or semi-permanent pair-bond;
> 7 the migration destination is within the combined familiar area of the pair and should be the destination that is assessed to be most suitable;
> 8 net rate of energy uptake per individual and deme size and density are the criteria on the basis of which pairs are most likely to assess habitat suitability, with greater weight being given to the former (Baker 1978:120).

Implicit in much of Baker's model is the importance of reproductive strategies. These same considerations which govern, in part, his model of human dispersion patterns have also successfully been employed by others to support the need for formation of various types of seasonal and long-term groups observed ethnographically for small-scale societies. At the most fundamental level, grouping tendencies can be viewed as originating as a result of subsistence or reproductive strategies.

> According to the anthropological and ecological literature, several factors may explain the relationship between group size and subsistence behavior. Briefly, the most important parameters are: resource density, resource nucleation, resource reliability, the importance of hunting, the nature of game hunted, and food-sharing practices... Important reasons lead me to expect that the most efficient subsistence economy occurs in groups of minimum size... Because substantial energy is used in travel and transportation of food... large aggregations of people are likely to rapidly exhaust food resources within the foraging range of campsites, thereby forcing the entire group to move frequently. The smaller the group, the less often it must move, and the less travel and transport work it must perform. Settlement stability is particularly important for women, who usually carry children and household materials during camp moves and must erect shelters (Hayden 1981:360) [emphasis in original]

Although Hayden's comments are based on a survey and analysis of the ethnographic literature, his conclusions fit very well with the predictions which one might offer on the basis of optimal foraging theory, and the earlier discussions regarding individual as opposed to systemic forms of foraging among hunters and gatherers. Foraging in fine-grained habitats will be most efficient when the foragers disperse. Ethological observations yielding comparisons of the foraging behaviors of gorillas and chimpanzees tend to confirm this same conclusion (cf. Reynolds 1979:335-336). Low productivity habitats will be less able to support a spatial concentration of foragers, resulting in smaller groups and more frequent group relocations (e.g., among the Washo [Downs 1966], Western Desert aborigines of Australia [Gould 1981], and the G/wi Bushmen [Silberbauer 1981]). Systemic hunting of larger prey will benefit by a larger volume of more current information about the natural habitat, while some prey types are more easily obtained through cooperative hunting. Increased active hunting, cooperation, and information sharing by hunters will tend to achieve these goals more efficiently.

Given the fundamental nature of human adaptations, both the reliance on systemic forms of foraging (in addition to individual foraging) coupled with food sharing, as well as the relatively slow ontogenic development and the requirements of mating and cooperative defense, all combine to dictate the adoption of some form of social group living for _Homo sapiens_. Martin Wobst (1976:51) concludes from a survey of the ethnographic literature that band level organization typically ranges between 15 to 75 individuals, with a modal value of 25. Wobst's model focuses on the establishment of mating networks ranging in size from 175 to 475 individuals of all ages, arranged in bands averaging 25 individuals (Wobst 1976:50). The geographical distribution of these bands is modeled using a Loschian system of hexagonal areas well known in geographical modeling (cf. Haggett 1965).

Although the numbers employed by the model output are based on ethnographic observation, the theoretical projection of band demographic parameters which assume as their prerequisite that reproductive individuals will be able to find suitable mates efficiently, results in some far-reaching implications. The local mating network becomes a biological unit; a cultural unit tied by kinship, language, and social bonds; and it also becomes an effective information unit, the expressions of which include (but are not limited to) shared subsistence and technological traits (cf. Wobst 1976; Bettinger 1980; and Root 1983).

> Information is a resource. It is something that must be gathered either directly through searching the environment or indirectly through the social processes that distribute information among the individual decision makers... Quite simply, information is not free in terms of acquisition costs, it is not distributed evenly over the social or physical environment, nor can it be obtained without creating constraints on the range of future actions (Moore 1983:178).

Viewed from this perspective, the model utilized by Wobst can be seen as a theoretical device which clarifies the acquisition, sharing, and maintenance of a fundamental and highly significant resource in small-scale societies—namely, habitat and subsistence related information.

Discussing the flow of information as modeled by Wobst, Dolores Root notes:

> Information is an intrinsic aspect of social relations of production where processing, storing, and transmitting information is essential for the reproduction of social relations. Material flows often accompany information exchange, and these produce structure in the archaeological record. Therefore, the social relations of production can be tracked in the archaeological record by examining the material expressions of information. Information is encoded in or on artifacts and this accounts for a large proportion of the variation in material culture (Root 1983:208-209).

It is precisely the multiple implications of Wobst's model, (addressing as it does territoriality, mating patterns, demographic composition, group size, population densities, information sharing, and other cultural and technological bonds) which makes it probably one of the more attractive approaches toward modeling small-scale societies. This statement is not to imply that the model output can be accepted as it exists, without any reservations. Furthermore, one should keep in mind that however attractive, Wobst's model is a heuristic device which should not be confused with a blueprint for practical archaeological applications. In addition, as Bettinger notes:

> Wobst's research is clearly important because it provides concrete estimates of the minimum numbers needed to maintain viable population units. At the same time, one can raise several objections to both his conclusions and assumptions. For one thing, his explanation of the minimum band 'magic number' of 25 does not show why its discovered 'half-life' is critical to human adaptation, nor does it show that a group of 25 is in fact more capable of satisfying subsistence tasks or less likely to overtax local resources than any other number... More to the point, however, hunter-gatherers typically draw upon resources that are highly concentrated in some places and almost absent in others; their population densities parallel these distributions. For individuals within such resource concentrations, potential mates are most likely to be available within the same resource concentration, so that distance between mates ordinarily does not work the hardship Wobst assumes... To close this, I would also point out that while Wobst takes increasing distance between potential mates to be a disadvantage, others have pointed out that it may be an advantage since it increases the probability that should one's local resources fail there will be access to the unaffected resources of a relative living a substantial distance away;...settlement in linear environments, therefore would seem to be less affected by distance penalties than Wobst supposes (Bettinger 1980:230).

In reference to these criticisms it might be well to remember that Wobst acknowledges that the "magic number" 25 which he uses in his model output is derived from the average band size reported from ethnographic accounts, and that Wobst acknowledges that variations in this number range between 15 to 75 individuals (see above). As Bettinger and others (e.g., Hayden, above) note, environmental variations in resource concentration can directly influence the size of the local population which can be supported in any specific situation.

Furthermore, I suspect (though at this point it would be difficult to support this conclusion quantitatively) that variations in the proportion of a small-scale society's total energy budget derived from individual versus systemic foraging is another highly significant component in the final equation expressed by the numbers of individuals which typically constitute such ethnographically observed groups. In effect, a heavier reliance on more complex systemic forms of foraging is likely to be aimed at larger or more "concentrated" prey energy sources, or perhaps a wider variety of such prey types, which in turn may only be accessible through the cooperative interactions of larger forager aggregates. Conversely, increased reliance on energy receipts obtained through individual foraging modes will relax these grouping requirements while permitting a more efficient means of accessing smaller, and more dispersed food sources in a fine-grained environment. These interactions are in many ways unique to individual situations, actively preventing any attempt at more detailed predictions at the collective level of "small-scale societies" or "hunters and gatherers" in general.

Bettinger's other criticism of Wobst's model relating to the advantages of finding mates from more distant areas, on the other hand, is well taken. One might also argue that there is no reason to suppose that hunters and gatherers maintain hexagonal territories. This is an artifact of modern perspective adopted in order to simplify an otherwise highly complex set of phenomena in the world of reality. Likewise, the successful maintenance of mating networks over linear environments constitutes a departure from the predictions of the model output.

Still, it might be obvious that we can view these examples only as departures or exceptions in light of the very existence of a model such as the one proposed by Wobst. In other words, the value of a model lies not only in what its model output successfully predicts, but also in understanding the manner and the reasons behind instances in which it fails to predict reality correctly. This represents the heuristic dimension of models such as the one proposed by Wobst (1976).

The recognition that the model presented by Wobst is intimately tied to the way in which individuals and groups access and control information allows us to relate this model to a discussion of territoriality presented by Elizabeth Cashdan (1983). Since group size can be related to resource availability and density, and assuming that local groups will benefit by establishing and maintaining some level of exclusive territorial rights, Cashdan begins with a review of the principles developed in evolutionary ecology relative to territoriality.

In her review, Cashdan notes that as the productivity of a habitat decreases and the size of the overall territory required to maintain the energy needs of a group increases, it becomes progressively less likely that a group could efficiently or effectively defend its territory. Speaking of human hunters and gatherers in this situation, Cashdan observes:

> As resources become sparse or unpredictable and ranges become larger, making perimeter defense uneconomical, humans have another option available. Rather than give up territorial defense entirely, they can control access to the social group inhabiting the area (social boundary defense) (Cashdan 1983:49).

Posing the question, "Why trouble to cultivate rights of access if one can take resources without asking?" Cashdan says:

> One reason is the importance of information exchange in these environments. Where territories are large and resources unpredictable, outsiders can minimize their foraging costs considerably if they obtain information from the residents concerning the location of resources. They therefore have an incentive to seek access to the social group, since it is through the social interactions of these rituals that information exchange takes place. To the extent that this is true, furthermore, we can expect that the costs of not cooperating in asking permission will rise with territory size, since the more unpredictable the resources and the larger the territory, the more difficult it will be for outsiders to exploit the area unaided (Cashdan 1983:50) [emphasis in original].

Viewed in this light, mating networks modeled by Wobst acquire a new meaning. By establishing and maintaining such interactive networks, small-scale societies continually exchange valuable information which enhances each group's ability to survive in a particular set of environmental situations, while allowing them to exclude outsiders simply by outcompeting uninvited transgressors. Furthermore, although Cashdan clearly intended to focus this explanation on special situations in which resources were unpredictable or in which habitat productivity was low, there is no compelling reason why these same mechanisms might not operate in similar ways (though perhaps not as effectively) in more stable or more productive habitats. Naturally, the process of territorial defense (as described above) assumes the presence of an extant set of populations, each living within territories which are stochastically and ecologically distinctive in their features.

In any event, Cashdan's emphasis of the value of habitat-specific information in promoting the survival of small-scale societies underscores the statements presented above by Moore (1983) and Root (1983). Furthermore, the key role ascribed to information in these instances ties in well with discussions of the nature of systems, and specifically cultural systems, which maintain homeostasis by cycling information (from and to their environments) along with materials and energy.

The continued success of a human group of hunters and gatherers, expressed as reproductive success or the maintenance of critical biomass, is clearly dependant on the quality and quantity of the information available to the group. When seen in this light, Baker's (1978) eight predictions regarding hunter and gatherer dispersal behavior (above), acquire a new level of meaning. Furthermore, although the systemic foraging behavior represented by male hunting often may not provide energy returns equivalent to female gathering activities, the increased range of these activities in less frequented areas of the habitat may produce a wealth of detailed habitat-specific information which may well help to offset the difference in net energy returns. Such information, of both short and long term value, may form the basis whereby central place relocation decisions can be arrived at efficiently, while providing critical knowledge which could prove invaluable to a group during periods of scarcity.

3.7 SMALL-SCALE SOCIETIES: CULTURAL STABILITY AND CHANGE

Despite the use of general ethnographic analogy and the emphasis on general ecological and biological principles in modeling key features of small-scale societies, we must recognize that such an approach avoids consideration of the unique features which distinguish individual cultures. No two cultures are identical, just as no two individuals are the same. Our ability to model small-scale societies through unifying principles should not be confused with a failure to recognize the importance of the distinctive traits which set them apart.

Throughout the preceding sections of this chapter I have approached the process of modeling significant features of small-scale societies from essentially a synchronic perspective. This is not surprising since much of the ethnographic and ecological data used in support of the preceding discussions was generated from short-term or synchronic contexts. Therefore it is expectable that issues revolving around proximate mechanisms should dominate such discussions.

In the context of largely synchronic discussions it is also expectable that comparison and contrast might serve to highlight both processual similarities and individual differences between small-scale societies. On the other hand, however we may choose to model the cultural dynamics which characterize small-scale societies, we must clearly distinguish questions of how similarities and differences can be explained, from questions which ask why these same similarities and differences came about. These questions highlight the fundamental set of distinctions discussed by Dunnel (1982:8) when he subdivided the sciences into those fields which view reality from a "space-like" frame as opposed to those fields which view reality from a "time-like" frame. The approach and the answers developed by "space-like" frame sciences to answer questions related to how phenomena can be explained are not intrinsically better or worse than the approaches used by the "time-like" frame sciences in answering questions related to why particular realities or phenomena arose; they are simply different approaches with different explanatory goals.

Evolutionary biology has successfully employed the synchronic mechanism of natural selection and the observations of ecologists to develop largely diachronic explanations of the fossil record. This last subsection focusing on cultural stability and change in small-scale societies represents a first step towards incorporating synchronic observations to explain the essentially diachronic phenomena documented by the archaeological record. In this case, unlike evolutionary biology, the focus of discussion must address culture, and the cultural dynamics of small-scale societies from a diachronic perspective. Therefore, this section introduces a shift towards diachronic concerns with a discussion of cultural stability and cultural change in small-scale societies.

> Whether we like it or not, changes are constantly taking place. Sometimes they transform whole ways of life and basic relationships between humans and between society and nature, as did those changes resulting from the Industrial Revolution. Or they may be a less obvious kind, such as modifications in the culture that a new generation always introduces (Spindler 1977:3).

When applied to our modern industrialized world, such statements have an almost self-evident quality. However, these statements often stand in sharp contrast to the way we view small-scale societies. Renato Rosaldo has argued that "...the image of the timeless primitive is not a finding, but an illusion created by the preeminent methods of anthropological research" (Rosaldo 1980:1). Rosaldo feels that the illusion of timeless stability can be traced to the essentially synchronic approach of ethnographic data gathering which are then used "...to deduce features of long-term social structure from empirical observations made within a narrow slice in time" (Rosaldo 1980:10).

Rosaldo makes clear that our perception of timeless cultural stability among "primitives" or small-scale societies can be traced to two related mechanisms. The first of these obtains as a result of the essentially synchronic perspective which accompanies our ethnographic data base. Not only is this perspective a result of the short length of our field observations, but also it has become a preconceived expectation which becomes expressed in ethnographic reports. As Rosaldo puts it,

> The extent to which the fiction of the timeless untouched people has guided anthropological research, especially of the so-called primitive variety, can be judged by the number of ethnographies that begin with prefaces designed to establish that a particular group of people has long lived in remote isolation (Rosaldo 1980:25).

The second mechanism which has given us the "illusion of timelessness" stems from a perspective which emphasizes the structure or cognitive map of a culture while subordinating individuals and their behaviors to the role of participants in an otherwise static and predetermined cultural "script." As with theatrical drama, particular actors will contribute individuality to their respective roles, while the play itself remains unchanged. Despite cultural norms, individual behaviors are often unpredictable (cf. Spindler 1977:5).

To these and similar perspectives Rosaldo responds by noting that they fail to perceive that "...human life is both given and actively constructed" (Rosaldo 1980:14) [emphasis in original]. Elsewhere he notes:

> Indeed, among the Ilongots, owing in large measure to the absence of a hierarchical ordering of social life beyond sex and age, no single official version of events is widely accepted, and in this sense their cultural knowledge is deeply perspectival.
>
> In general, I am convinced that the central issues of social thought are not only the conventional ones of cohesion and the normative order, but also the emergent ones of dissension, and the politics of persuasion, both artful and forceful. To say that certain rules are not given but require interpretation is to say that the study of cultural processes requires an investigation of the structural position of each particular rule-giver. A number of social processes are thus ordered, less through the simple realization of structural principles than through struggles among people with different stakes in the outcome of events (Rosaldo 1980:20-21).

It is my perspective that these observations help to highlight the inherent limitations of a static structural approach to cultural analysis—particularly when dealing with processual issues. After all, culture is always dynamic; culture is a process, not a thing. However real cultural norms and structures may seem to be to an individual (ethnographer or informant) at any one point in time, the components of any culture should always be viewed as actively engaged in a process of "becoming" —perhaps slower to change than our perceptions or expectations will allow, yet never static or timeless.

In 1953 Homer Barnett approached the question of the origins of culture change as follows:

> All cultural changes are initiated by individuals. The stimulus for a new idea or a new behavior is consequently always specific to a given individual. Sometimes, however, it is generalized so that it bears upon several individuals, or it becomes more intense or recurrent so that it affects some individuals more acutely than it does others. In any event, conditions external to the individual have a marked effect upon his innovative potential and upon the potential of the group in which he lives. With a shift of conditions in the experience of an individual, or with an alteration in the conditions that affect one society at different times in

> its history, or with a diversification of the conditions that affect different societies, there must go a variation in innovative possibilities (Barnett 1953:39).

The individual, then, becomes the focus of cultural changes. It is precisely individual variability which brings about cultural change. And yet, no two individuals perceive the world, themselves, their culture, or their position in their culture in quite the same way. Individual variation, then, is the norm, rather than the exception.

In a related and pertinent discussion, Robert Hall observes that

> It might be helpful to have a term, parallel to _idiolect_, to cover all the cultural patterns of an individual, defining these latter negatively as everything in his behavior which is not conditioned by heredity or physical environment; _idiocult_ seems awkward, but may do until some-one can suggest a better term. A 'culture,' in the sense of the behavior of a community, is only as much as is common to two or more idiocults. A cultural innovation is, therefore, part of cultural behavior from its inception, insofar as it forms part of the idiocult of its inventor; then, of course, it becomes part of the 'culture' of the group as soon as at least one other person has taken it over. The concept of the idiolect can thus help to establish the parallel notion of 'idiocult' and relieve anthropology of a perennial, but needless logomachy over the nature of 'culture' in the abstract (Hall 1967:398-399) [emphasis in original].

To speak of culture from Hall's perspective in the context of large-scale, complex, hierarchically organized societies borders on accepting a trite definition of what most anthropologists intend in their use of the term culture. On the other hand, seeking to establish concrete, long-standing, and static or nearly static structures and norms in the definition of the culture of small-scale societies appears to be equally inappropriate. Our point of reference, in this latter instance, appears to have a major role in adopting such a mistaken view. Rosaldo has put this problem in its proper perspective when he wrote,

> What is most difficult to discern in the study of so-called primitives is the extent to which anything of significance has changed. In such communities, so remote and perhaps alien from our own, the problem is to determine precisely the kinds of change that in fact do take place through time. For those of us rather coarsely attuned to more strident movements—the rise and fall of massive totalitarian regimes, the widely resounding booms and busts of the stock market—Ilongot history must appear comparatively silent, even immobile. Relative to our own, the Ilongot past has been inscribed in miniature, written in such a low key that its moving rhythms are all too likely to elude us. Our moral imperative, in attempting to comprehend Ilongots,

> should be that we take their lives nearly as seriously as we take our own. The perception required of us is that seemingly small changes, many of them phrased in an unfamiliar cultural idiom, can have vast reverberations in the lives of people (Rosaldo 1980:27-28).

For all its potential drawbacks, Hall's perspective appears to be more appropriate as a point of departure in discussing culture change in small-scale societies, than does the more widespread and traditional anthropological perspective which focuses on the definition of a backdrop of static cultural norms and blueprints against which individual variability (Hall's "idiocult") is reduced in importance and relegated to the status of inconsequential "deviations" from some norm.

In a small-scale society composed of twenty-five individuals, Hall's minimum number of two individuals sharing the same "idiocult" represents eight percent of the total population. By comparison, this same proportion of the population of the United States would equal nearly twenty million persons. A cultural trait or traits shared by twenty million persons, even from the perspective of the vast population of the United States, would surely constitute a valid starting point for the definition of some type of a sub-culture.

I hasten to point out that it is not my intention to argue that we should formalize cultural traits shared by two individuals in small-scale societies with the label "sub-culture." Rather, I merely wish to indicate that the perspective one adopts in defining the nature of culture, and the impact which "idiocult" variations have upon cultural variability and culture change is highly sensitive to the overall size of the cultural unit under discussion. The individual, together with his or her unique perspectives, attitudes, interests, and behaviors has a much greater impact on the nature of a small-scale society's culture than it does in the context of a large-scale, complex, hierarchically organized culture. It is precisely this realization which makes Hall's perspective more appropriate as a starting point in any discussion of culture change among members of small-scale societies. This is the perspective which I plan to use in the following discussion.

At its simplest level there are three key ingredients whose interplay results in cultural stability or change. These ingredients include 1) the individual human beings who produce and participate in a specific culture; 2) the natural and cultural environment; and 3) the cultural system itself.

In the preceding discussion I have focused on role and contribution of individuals to the dynamic equation which results in cultural stability or change. In the context of small-scale societies the focus on the contribution of individuals to the total equation becomes especially important due to the proportionately greater influence of each, relative to the whole.

Beyond these considerations, there is a vast literature in anthropology addressing the impact of external influences on the occurrence, manner, and rate of culture change—particularly as it pertains to culture contact, borrowing, diffusion, and acculturation. I do not intend to address these topics here. The role played by changes in the external, natural environment in bringing about culture change have also received extensive treatment in various forms—from the early ideas of environmental determinists to the holistic and dynamic model of the culture core presented by Julian Steward (1955).

Clearly, as Woods (1975:11) notes, "...migration to a new area, natural catastrophes, and climatic changes would represent modifications in the physical environment." These, in turn, will provide the stimulus for culture change, as dynamic relationships become altered. However, as Spindler writes,

> The natural environment does set limits, especially for people without advanced technology in severe environments. However, the environment alone cannot determine the nature of a particular system. The interpretation of the environment in terms of a group's particular belief system and values, makes the environment something different for each group. There is no one 'best' solution to the problems posed by the natural environment (Spindler 1977:25).

Whereas major environmental changes resulting from drastic climatic shifts, migrations, and natural catastrophes will clearly illustrate the relationships between external stimuli and culture change, I would argue that we must broaden our perspectives to include much more subtle and on-going changes in the external environment which—though perhaps less dramatic—are nevertheless more typical and widespread. Spindler's approach, highlighting as it does the effect of "severe environments," deserves a similar comment.

All cultural systems maintain a dynamically interactive relationship with particular external environments requiring investments of energy by the cultural system. "Severe environments" presumably require greater energy investments by the cultural system. In a small-scale society the increased energy demands of "severe environments" are more keenly felt. The need for increased energy flow and increased efficiency is more immediate, restricting more severely the range of acceptable choices and the degree of variability which the cultural system can tolerate while still meeting the biological and cultural energy needs of the population.

The dynamic interactions which ultimately permit a discussion of culture change occur at several component levels. The minimal per capita biological requirements of members of a local population must be met. These requirements, by themselves, vary in direct linear proportion to the number of individuals composing the population. Different natural environments can be demonstrated to contain varying though always finite quantities of material and energy. The rate at which these can be cycled and replaced varies considerably between environments. Furthermore, the environment's resistance to any system's capacity to access materials and energy from the natural environment increases disproportionately as the upper limits values are approached.

As increasing population numbers place greater demands on the cultural system to access materials and energy from the environment, the cultural system can only meet these increasing demands (which are accompanied by increased environmental resistance) through systemic reorganization and increasing systemic complexity (socioeconomic and technological). Increased systemic complexity, in turn, requires a disproportionately escalating diversion of the cultural system's total energy budget toward meeting systemic needs (above and beyond the biological needs of the population). The extension of these trends will encounter a point where diminishing returns (in the context of local conditions) will directly limit both population numbers and viable culture-systemic alternatives.

Turning to the issue of "idiocults" and other forms of internal cultural variability within the context of the present discussion, we should note that 1) all cultural behavior has an energy cost associated with it; 2) each variant behavior can be categorized as representing a smaller or larger energy expenditure; and 3) insofar as behavioral variants result in gaining access to materials and energy from the natural environment, each variant behavior (or behavioral complex) will gain or lose for the system qualitatively and quantitatively different amounts of materials and energy. As a result we may predict that both environmental parameters and population size will influence the nature and incidence of cultural variability.

By definition, "systemic stress" will result when the biological requirements of the population or the operational and homeostatic requirements of the cultural system cannot be adequately met. Small-scale societies are more immediately responsive to systemic stress because the cultural system's energy budget forms a direct continuum with the energy allocated to the production and maintenance of human biomass.

The incidence of systemic stress will result in decreased internal variability in small-scale societies resulting in a set of conditions which Bicchieri (1969) has labeled as "restrictive." In contrast to "restrictive" relationships, "permissive" relationships between specific cultural systems, populations, and environments can be defined as ones in which the consequences of alternative behavioral variants and relatively widespread internal cultural variability can be homeostatically absorbed by the cultural system's total energy budget without creating significant systemic or human biological stress.

Although Bicchieri's original usage of these terms appears to be directed toward the emic definition of a group's habitat or environment (Bicchieri 1969:177), his subsequent approach (cf. Bicchieri 1990) makes it clear that his orientation incorporates a set of multifaceted dynamic relationships (similar to my own, above), rather than being limited to the more narrow usage which formed the focus of his earlier work. However useful the contrasting extremes represented by the terms "restrictive" and "permissive" might be, we should be careful to conceptualize them as relative terms in a variable scale which describes only one of many axes influencing the expression of cultural variability in a vast variety of actual cultures and culture systemic states.

In most societies, culture change is a normal, and expectable process. Culture change is also a complex phenomenon which represents the composite interaction of many factors resulting in qualitatively and quantitatively different outcomes. The use of a single label ("culture change") tends to obscure the complexity of these factors. Unfortunately, the "narrow slice of time" which is represented in human memory or in largely synchronic ethnographic accounts seems to argue against the frequency or the fundamental importance of cultural changes. Finally, the relatively minor impact of "idiocults" in the context of large-scale industrialized societies with which we are most familiar, represents a poor source of analogues for the understanding the impact of individual "idiocults" in the context of small-scale societies. When we take into account the combination of these factors, an entirely new perspective on cultural variability and culture change among small-scale societies becomes possible.

Individual variability—Hall's "idiocult"—is a normal, constant, and expectable source of internal variability within all cultural contexts. In small-scale societies, the disproportionately large contribution of the individual relative to a small group elevates the significance of this source of variability to the status of a major dynamic component of culture change. The importance of "idiocults" is increased further by the action of preferential lines of transgenerational enculturation along kinship lines.

The dynamic relationships between specific environmental characteristics, specific cultural systems, and specific population densities will result in different levels of allowable internal variability from "permissive" to "restrictive" conditions. These specific dynamic outcomes will serve to moderate the expression of "idiocult" variability which can be tolerated by small-scale societies within specific dynamic contexts.

Without discounting the vast and complex impacts brought about by cultural factors such as borrowing, diffusion, and acculturation, we may note that both large and on-going small changes in the natural environment will also result in dynamic shifts in how specific groups interact with the physical environment. Cultural systems will continuously respond to on-going environmental changes. Responding to specific local conditions, small-scale societies are more likely to show significant inter-group variability, local identity, and maintenance of systemic segmentation over larger geographical areas. In the face of these factors, mating networks, trade, and diffusion become powerful cultural mechanisms counteracting the diversifying effects of local environments.

Additionally, ecologically-complex environments can also encourage internal cultural variability in two ways. First, varied resources present a variety of problems which often require different specific solutions. Second, the adoption of a diverse set of responses aimed at meeting biological and systemic needs through a broad-niche adaptation tends to preclude the development of any single, highly efficient systemic response which could easily displace other responses. The dynamic feedback of these types of interactions results in the maintenance of a more stable population density over time at a level which tends toward the "permissive" end of the dynamic range of possibilities. The "permissive" quality of these relationships, in turn, closes a mutual feedback loop of relationships by encouraging internal cultural variability.

I wish to close this discussion by examining one final, and special type of culture change among small-scale societies. The mechanisms which introduce change in this instance are threefold and affect small-scale societies undergoing group fission. A detailed description of these effects have been described by Hammel and Howell (1987:146-47) as follows:

> What are the consequences of fission? The first is that the daughter groups are smaller than the parent group. As such they are more subject to the vagaries of chance; their demographic rates become unstable simply by virtue of their smaller size, so that they are more likely to begin precipitous declines or sharp accelerations in size. If groups decline in size, they can of course become extinct, thereby creating gaps in the network of communication that increase physical and cultural differentiation among those that survive... A second consequence of fission is that smaller size combined with geographical separation makes it harder to find mates, which condition may contribute to the reduction of growth or, alternatively, increase the probability of cultural change in rules of mate selection or the development of kinship nomenclatural systems ensuring at least one incumbent in each named kinship role... A third consequence of fission is that daughter societies will not be perfect copies of the parent society. Not for a very long time (if ever) has every adult in a human group known exactly as much as every other. On fission, some knowledge is lost. It may of course be regained by reinvention, but this may be as unlikely as repeat mutation recovering a lost biological characteristic. (Since people began to think systematically about problem-solving algorithms, the probability of repetition of identical solutions has been enhanced)... Most interesting, new solutions for old problems may emerge. Some of these may work worse than earlier solutions, but some may work better. In fact some may overcome the resource limitations that earlier forced fission.

As local populations increase, resulting in more restrictive conditions, one possible alternative to counteract systemic stress might be group fission accompanied by the establishment of separate territories, a development which might also involve some form of group emigration. Hammel and Howell's first two consequences of this process require little elaboration. The third consequence, however, has numerous other potential ramifications.

Whereas Hammel and Howell focus on the loss of knowledge which results from the fission of a group in which each individual possesses less than the sum total of the group's knowledge, we may also note that internal social ties will also encourage the formation of daughter groups whose members share more details in common within each group than between daughter groups. Social and kin ties associated with non-random preferential enculturation pathways and interactive pathways will serve to encourage such unrepresentative composition of individuals and knowledge in the resultant daughter groups. To some extent, not only knowledge, but the details of the problem-solving algorithms employed by each daughter group will also tend to differ between the daughter groups produced through fission.

Alternatively, one may focus on a process of fissioning in which the non-random division takes place among groups of different mean ages. As discussed in an earlier section, by analogy to mammalian dispersal patterns, including pre-saturation dispersals, often it is the younger members of a community which emigrate out of their natal territories. Once again, if one were to model fission in small-scale human societies along these lines, the loss of knowledge in the resultant emigrant group, coupled with the increased plasticity of behavior among younger individuals, would likely contribute to the production of contrastive differences between the resultant groups.

Another point which should be made relates to a sequence of effects which brings about differences between such daughter groups. Fission accompanied by emigration and a new set of environmental relationships will likely result in a more permissive set of conditions. These new conditions, in turn, will make more internal variability possible within each daughter group. The increase in more internal variability, coupled with new environmental conditions and problems, will directly contribute to the generation of new solutions, making additional culture change possible and likely.

Related observations to those presented by Hammel and Howell or in the subsequent discussion (above) have also been presented by Keegan and Diamond (1987:74) when they write:

> Any colonization of an island, whether by humans or by other species, confronts a series of demographic and ecological problems. First, the colonized island's environment will generally not be identical to the colonist's source environment. To exploit the new environment, the colonists must begin to make ecological adjustments or niche shifts. Second, the initial group of colonists is generally few in number and exposed to a high stochastic risk of extinction.

Although group fission is not limited to the establishment of a daughter population as colonists in the context of islands, there are valid reasons to consider the special conditions attendant to island colonization in the present context. The emphasis here is on the external influences affecting a daughter group. In some ways, physical isolation and environmental differences present an extreme case which help to accentuate the contribution of these factors in bringing about culture change. Otherwise, fission which results in the formation of daughter groups separated by less formidable barriers and involving more subtle environmental differences involves much the same factors as island colonization—though perhaps in less extreme form. Both examples result in some level of differentiation, isolation, obligatory adjustments to new environmental conditions, and ultimately culture change.

As discussed earlier, the principal aim of this chapter was to formally introduce and discuss a range of topics and operant principles which will be used in the to construction of the model output of Proto-American prehistory. This chapter discussed a number of topics and principles related to the cultural dynamics of subsistence, division of labor, resource allocation, ecological factors, reproductive and demographic alternatives, enculturation, group cohesion and fissioning, and culture change within the context of small-scale societies. The data, and the principles used in modeling the dynamics of these factors, were derived from the published literature in anthropology, biology, and demography.

These sources and discussions were presented from an essentially synchronic perspective. In order to address diachronic issues, this work has adopted a uniformitarianist perspective to the use of general ethnographic analogy, and to the applicability of various principles derived from ecology, demography, and evolutionary biology. One fundamental approach which might be used profitably to address diachronic issues involves viewing synchronic cultural processes through the perspectives of long-term culture change. This is the perspective which I have adopted in generating the model output presented in chapter five.

Whereas this chapter addressed a range of relevant processual issues, the next chapter attempts to establish the human biological, environmental, and temporal context of Proto-American prehistory. Thus, the next chapter shifts the focus of discussions from issues of "how," and "why," to issues related to "who," "when," and "where." In as much as possible, all of these issues must be addressed to the exclusion of the archaeological data of Proto-American prehistory, in order to formulate a comprehensive model output which avoids the difficulties associated with tautological arguments.

Chapter Four

INDEPENDENT DATA AND PERSPECTIVES ON PROTO-AMERICAN PREHISTORY

It has been my contention herein and elsewhere [Alsoszatai-Petheo 1986] that the manner in which we define the relevant questions pertinent to the initial peopling of the Americas is just as important as our efforts in gathering and analyzing related data. Should we choose to define the topic as mostly or exclusively an archaeological problem, our solutions—together with the practical and epistemological limitations of such an approach—would likely produce results which could only be falsified or corroborated directly by additional archaeological tests. The danger of this approach is that it would fail to circumvent any systematic bias which might be inherent in the logic or data base of this single approach, while depriving archaeologists of the inputs and testing opportunities afforded by wholly independent lines of inquiry.

The need to approach the study of Proto-American prehistory from a comprehensive [holistic] biocultural perspective was discussed in chapter two; while the last chapter integrated biological, ecological, and cultural processes and principles into a series of interdependent constructs. Indeed the deliberate use of the term "Proto-American prehistory" [defined in chapter one as the study of "the initial peopling and early prehistory of the Americas"] was introduced in this work to highlight the comprehensive biocultural approach which characterizes this work.

The discussions which follow in this chapter are presented to broaden the basis of this approach by introducing independent data and arguments derived from other disciplines and subdisciplines. The inclusion of these data and arguments does not represent a dismissal or a replacement of more traditional archaeological approaches to problem resolution. Instead, I believe that the data and perspectives which I intend to propose in this chapter represent separate legitimate realms of inquiry in their own right which share a common focus in the factors surrounding the initial peopling and early prehistory of the Americas.

Independent data and conclusions of related fields and subfields can serve to establish an integrated context from which a heuristic theoretical model output of Proto-American prehistory can be generated. An understanding of the pertinent aspects of the biocultural dynamics of small-scale societies is only one component. Additionally we must incorporate three other key sources of data and inferences. These sources include 1] a wide range of specific data derived from physical anthropology and demography designed to address population and chronologic issues; 2] data on paleoenvironmental parameters aimed at establishing aspects of the natural environment with which early populations must have interacted; and 3] archaeological data from East Asian and western Pacific rim sites which provide additional specific data on cultural adaptations and populational distributions of potential ancestral populations. The subdivisions of this chapter reflect this tripartite focus within an overall biogeographic and biocultural perspective.

It is my hope that the conjunction of these lines of inquiry, proposed from a biocultural perspective, will permit me to generate a model output of early American prehistory independent of the data and conclusions of American archaeology—yet capable of supporting dependent hypotheses which can be tested against the archaeological data base of Proto-American prehistory. There are numerous topics and issues which I believe could be addressed and perhaps answered from non-archaeological avenues of inquiry. The present chapter is not exhaustive. This chapter represents a first step toward the adoption of the broader biocultural strategy of Proto-American prehistory. The principal aim of this chapter is limited to providing discussions on a variety of specific topics which are necessary to establish the temporal, spatial, paleoenvironmental, and demographic context of Proto-American prehistory presented in the model output.

4.1 BIOGEOGRAPHICAL PERSPECTIVES

Outside the narrow confine of topics which form the traditional basis of the archaeological debate regarding Proto-American prehistory, the entry, spread, and settlement of the Americas by _H. sapiens_ in the late Pleistocene clearly constitutes not only a cultural but a biological event of considerable importance to biogeography. To evaluate the validity of this claim and to appreciate the scope of biogeography, the following definition found in a standard text on biogeography will suffice:

> The study of the patterns of distribution of organisms in space and time is called _biogeography_. Biologists are nowadays rarely satisfied with the mere description of these patterns, and the biogeographer usually wants to discover which environmental factors are the ones that determine or limit the distribution of the species he studies. To do this he must draw on knowledge from the whole spectrum of the sciences of life and the environment, including geology, geophysics, climatology, palaeontology, plant and animal systematics and taxonomy, evolution, physiology, and ecology (Cox et al. 1976:1-2).

Although the approach of biogeography is multidisciplinary, the primary perspective of biogeography is biological. This predominantly biological perspective may be objectionable to some anthropologists who perceive human beings as distinctive and different from the rest of the living world; partly because people alone are _assumed_ to posses culture which mitigates the need for reliance on direct biological adaptations. It was precisely these concerns which Jared Diamond (1977:249) addressed as follows:

> 'But man is basically different from other animals.' So one is often warned when the possible relevance of animal findings to understanding man is suggested. In fact, however, every animal species is basically different from other ani-

mal species. Faced with this diversity, biogeography has developed a common framework for understanding such unlike distributions as those of giraffes, rose-breasted grosbeaks, cockroaches and oak trees, by seeking relations among key variables or processes. These variables or processes are ones that are relevant to understanding any species. Like other species, man reproduces, dies, disperses, exploits environments of varying stability and productivity, adapts phenotypically and genetically, is subject to intraspecific competition, and [still like other species] differs from other species in particular characteristics of each of these processes. These are the processes that provide the ingredients for biogeographic analysis.

Despite the multidisciplinary approach of biogeography, and despite the fact that human beings share important processes which can act as common denominators between ourselves and other species, the paradigms and theoretical links between anthropology and biogeography may not be obvious until we realize that much of the theoretical and paradigmatic components of biogeography are identical to the theoretical and paradigmatic elements which guide modern physical anthropology. The transformation of the subfield of physical anthropology in the last forty years since the publication of Washburn's [1951] classic article has resulted in the introduction of conceptual, theoretical, and paradigmatic elements from the biological sciences which effectively provide the logical link between the biological perspectives of biogeography and the theoretical perspectives of other anthropologists—including those interested in early American prehistory. This development is true not only in the adoption of evolutionary principles from biology, and in the rise of modern paleoanthropology [cf. Straus 1991], but also in the internal maturation of the perspectives of physical anthropology, which now include many more overtly biological perspectives to the definition of our species within the framework of larger natural systems [cf. Foley 1987; Harrison et al., 1988; and Moran 1982].

4.2 LAND, CLIMATE AND BIOTA - AN INTRODUCTION

The combined land mass of the Americas is equivalent to over 42 million square kilometers; that is to say two-sevenths, or slightly more than 28 percent of the total land surface of the earth [cf. Hoffman 1990:773; Showers 1989:24-25; and Rand McNally 1981:40x, 1965:vi-xv]. The geographic orientation of the two continents is unique in that its principal axis extends primarily north to south over approximately sixteen thousand kilometers. The Americas stretch from almost 55 degrees south latitude, to several continental points in excess of 70 degrees north latitude—well beyond the Arctic Circle. Major mountain chains with a primarily north-south orientation [including the Andes and the Rocky Mountains] serve to accentuate further the north-south orientation of the two continents.

The northern reaches of the North American continent are separated along an east-west axis by over 100 degrees of longitude from western Alaska to eastern Canada. This vast east-west continental expanse begins to narrow gradually to the south through the funnel shape of Mexico, culminating at its narrowest zone in Panama. Here, the land narrows to approximately sixty kilometers before joining the South American continent. This geographical bottleneck effectively serves to set apart the two continents, thus insuring the continued and severe (though not total) biotic isolation of the two continents from each other.

From a purely biological perspective, the sheer size and physical diversity of the Americas suggests the likelihood that the land must support a vast number of varied and distinctive ecosystems. This prediction is accentuated by the consequences of a principally north-south positioning of the continents which superimposes on the local topography latitudinal climatic gradations extending from the Arctic through the tropics to the frigid near-Antarctic conditions of Tierra del Fuego. Though not a barrier to life, the latitudinal climatic gradations have served further to segment biotic communities by restricting the range of plants and animals whose niches contain some narrow tolerances along one or more axes.

It might be well to remember at this point that the intent of this review is not to summarize biogeographic observations for their own sake, but rather to present such information in relation to the initial entry and spread of our species in the Americas. As Martin et al. (1985:15) have put it,

> American archaeologists have often utilized geographic regions in their analysis of Holocene archaeological data. However, the ecological features that characterize the Holocene biogeographic regions cannot be extrapolated to the late Pleistocene of the same areas (e.g., Eastern Woodlands, Great Plains, etc.).

Whereas typically the outline of major geographic observations presented above must have remained true for the Americas over much of the Quaternary, the definition of geographic regions, climatic conditions, and ecosystems must be approached from a much narrower chronological perspective. Undoubtedly (the chronological debate among archaeologists notwithstanding), some alternative and independent avenue must be sought to establish a chronological base line for further discussion. Any attempt directly to employ chronological estimates internally generated from the known data base of American archaeology would be a circuitous approach leading to a tautological trap which would subtly and effectively defeat the intent of providing an independent test to the reconstructions available in the literature of American archaeology. In order to avoid this trap, alternative approaches to establishing an independent chronological framework must be employed. Several avenues potentially capable of contributing to the establishment of an independent chronological framework will be explored in the next ten subsections of this chapter.

4.3 PHYSICAL ANTHROPOLOGY AND DEMOGRAPHY

Given the pervasive nature of current archaeological orientations toward the resolution of chronological issues, it is difficult even to imagine that independent, alternative approaches might be available. I believe that alternatives exist. One of the ways out of this dilemma lies with an understanding of the significance of the data base employed by physical anthropologists and demographers, pertinent factors of which will be reviewed.

The data base employed by physical anthropologists, human biologists, and demographers can also be used to address other significant concerns. The source and movement of ancient populations, and the relative size and rate of growth of extinct populations, all must be considered in relation to the cultural tradition, adaptations, and diversification which must have accompanied the spread of human populations throughout the Americas. The first subsections of this chapter will address some of these concerns.

4.3.1 NATIVE AMERICAN ORIGINS

There is a near-universal agreement among scholars about the Asiatic origin of Native American populations. In fact, many scholars dismiss serious consideration of the issue of origins with statements such as:

> There is no reasonable doubt as to the ultimate origin of the human population that finally covered the hemisphere. There is consensus among scholars that the first American was of Asian stock. Research in biology, language, and archeology demonstrates this; no space need be wasted here in presenting the varied evidence pointing to the one conclusion (Jennings 1978:1).

Similar statements appear in both older (Josephy 1970:11) and newer (Kirk and Szathmary 1985:ix) works as well.

Often, supporting arguments for the Asiatic origin of Native American populations have incorporated geographical considerations. Stewart (1973:5-9) discusses the nearness of the northeastern Asiatic mainland to Alaska as part of the supporting argument meant to establish the Asiatic origin of Native Americans; while at the same time pointing to the geographic "cul-de-sac" represented by the Western Hemisphere, which ultimately influenced population movements and the biological composition of human populations throughout the Americas.

Other frequently cited arguments in support of the Asiatic origins of Native American populations have invoked both modern, anthropometric, or "racial" considerations (cf. Brues 1977); or alternatively, discussions of the pertinent taxonomic and paleontological evidence (cf. Steward and Faron 1959:31; or Wormington 1957:1). In most cases the content of these arguments points out 1) the biological similarities between New World and Asiatic populations; 2) the absence of fossil forms in the Americas which might be classified as earlier precursors of H. sapiens; and 3) the taxonomic distance between our own species and the only other living primates native to the Americas, the Platyrrhine monkeys.

Still more recently, highly sophisticated comparative genetic studies such as the ones by Williams et al. (1985) and Schurr et al. (1990), together with the evidence from the analysis of dental traits (Turner 1985; 1986a), all strongly reinforce the conclusions reached by earlier researchers. In light of these data and arguments, and in the absence of contrary evidence, I shall treat the issue of the Asiatic origin of Native American populations as a given. In other words the modern Native American populations known to anthropologists, together with the fossil, physical evidence of their ancestors, all indicate that the native New World populations descended from one or more ancestral populations which originally inhabited some portion or portions of the far-eastern Asiatic mainland.

4.3.2 TEMPORAL CONTEXTS AND MORPHOLOGICAL COMPARISONS OF HUMAN SKELETAL REMAINS

The acceptance of the Asiatic origin of Native Americans can yield some additional conclusions which significantly bear upon the relative chronology of the peopling of the America. If one were to draw comparisons exclusively on the basis of known skeletal remains, it would be difficult to support the proposition that the Americas were peopled prior to the rise of Homo sapiens in the Far East. Lacking evidence which might indicate that earlier types of populations (e.g. Homo erectus or early archaic Homo sapiens) entered the New World, the above argument must stand by virtue of its parsimonious use of the currently available evidence.

In a recent book, Robert Wenke has phrased a variant of this classic line of argument as follows.

> By 100,000 years ago Old World peoples seem to have had all the skills at making clothes, fire, and tools that would have been necessary for the trek to the New World. But intensive research has failed to turn up any New World human skeletons that cannot be comfortably fitted within our own species, Homo sapiens sapiens. Because the first H. sapiens sapiens appear in the Near East and Europe about 35,000 years ago, we might assume that the aboriginal Americans must have come over since then, but we do not know when the first Homo sapiens sapiens appeared in the East Asian areas—they may have been there long before they were in the Near East or Europe (Wenke 1990:206).

Thus, this argument states that based on the available early skeletal remains, the initial peopling of the Americas must have occurred after the earliest dated appearance of Homo sapiens sapiens in East Asia.

Over the years a wide variety of competent observers have examined and reported the morphological traits associated with ancient human skeletal remains in the New World. From the pioneering work of

Hrdlicka to modern compendia such as the one presented by Protsch (1978), there seems to be little doubt that early American fossil specimens conform in their principal morphological details to the main outlines which characterize anatomically modern humans or Homo sapiens sapiens (cf. Stewart 1973, 1981; and Owen 1984. Also see Clark 1978:52-53, and Klein 1989:349-351 for discussions of pertinent anatomical traits of H. sapiens sapiens). The single critical exception to this generalization might be the skeletal remains recovered at Lagoa Santa, Brazil (Bryan 1978:318-321; Beattie and Bryan 1984:345-46).

Both Stewart (1973:155-158) and Bryan (1978:318-320) offer discussions of the history of the Lagoa Santa finds. Initial reports from the last century indicated that human skeletal remains had been found in stratigraphic association with the bones of extinct Pleistocene animals. Stewart's own application of fluorine dating confirmed that extinct animal bones and the human skeletal remains recovered from Confins Cave were of equivalent relative age (Stewart 1978:156).

Dozens of human skeletons had been recovered from caves in this area. Most of them, including Confins, have fully modern features, although some specimens from Sumidouro Cave appear to share the distinctive morphological features and the pattern of permineralization which characterize the calotte reported by Beattie and Bryan (1984). The limited nature of this sample, combined with the fact that the main calotte is not available for direct morphometric study (acknowledged by Bryan 1978, and Beattie and Bryan 1984), make it impossible to draw anything approaching an in-depth analysis of these finds. The comments in these two reports along with the published photographs and drawings permit, nonetheless, a brief consideration of their significance.

The Lagoa Santa calotte has been described by Beattie and Bryan (1984:346-46) as follows:

> The frontal bone possesses extremely thick and protruding supraorbital ridges formed by highly arching and symmetrical bars of material stretching from the frontozygomatic suture areas to a thickened confluence in the glabellar region. The massive nature of the arches has produced very rounded supraorbital margins and a well-defined postorbital sulcus. This sulcus is most pronounced laterally and is continuous across the midline... The exact locations of glabella and nasion are unclear because of the buildup of material in the interorbital area. The frontal arc is well rounded, producing a fairly high forehead, though the frontal eminences do not appear to be very prominent. There is no median frontal ridge. A possible supraorbital groove may be seen in the right orbit. The complete lack of temporal line development on the frontal and parietal bones indicates very thick surface-obscuring deposits in the areas peripheral to and including the supraorbital ridges. Slight parietal eminences can be seen, and a flattened obelion is evident.
>
> On the internal surface... an extremely thick and long frontal crest is present, ending posteriorly in a slight

> groove (superior sagittal sinus?). Sutures and arterial sulci are not visible. The remaining internal features (e.g., the grooves apparent in the left side of the frontal bone...) almost certainly represent postmortem damage. Other features include the remnant of the vertical plates of the ethmoid and frontal spines and the presence of diplöe along a partially damaged area of the left parietal.

Bryan (1978:320) provides some additional and significant observations. He observes that the calotte shows a "pronounced post-orbital constriction," and that the frontal bone was "decidedly sloping" although "the vault was not notably low." Bryan also points out that the specimen exhibits a low nasofrontal suture with a "very great" interorbital breadth.

Working from the published photographs (Bryan 1978, and Beattie and Bryan 1984), I would like to offer the following additional observations. The transverse outline of the calotte shows considerable rounding along the coronal plane, becoming more angular with a flattened superior surface in the region between obelion and lambda, and showing markedly broad and laterally sloping sides in the posterioparietal region.

The sagittal outline shows a similarly flattened area between (what appears to be) the opisthocranion and obelion. There is a noticeable (though not extreme) flattening along the anterior two-thirds of the sagittal suture which visibly departs from the degree of superior Sagittal rounding often found to be characteristic of modern populations. The outline of the frontal bone along the midsagittal plane becomes more curved, producing a distinctly rising forehead which nevertheless fails to produce a vertical or even a near vertical forehead. The anterior terminus of this line merges with the medial segment of extremely thick and well-rounded browridges continuing in an anteriorly sloping angle, downwards toward the nasal bones, suggesting that the nasal root would have shown little or no depression. There does not appear to be any suggestion of a ridge or keel anywhere along the midsagittal plane.

Given the obvious limitations of working from photographs taken from a partial specimen, it is impossible to calculate any of the craniometric indices with any real degree of certainty. Rough observations, however, suggest that the calotte appears to conform to dolichocranic proportions, with a relatively wide, low, and somewhat posterior placement of the euryon compared to modern populations.

On the basis of the published photographs and the composite description provided above, some general comparisons can be made to other fossil specimens from the western Pacific. Bryan (1978:321) states that the Kow Swamp population of Australia

> ...is morphologically amazingly similar to the Brazilian [Lagoa Santa] calotte in respect to the type of double-arched supraorbital torus, the broad interorbital combined with fairly low nasion, and the quite high and thick-walled vault... The Mapa skullcap from South China ...is also similar to the Brazilian calotte and appears from front view

> to be nearly identical to the Kow Swamp 1 cranium.

My own assessment of these comparisons differs somewhat from those of Bryan (above).

Although the Kow Swamp 1 cranium shares a number of archaic morphological details with earlier Far Eastern archaic *Homo sapiens* specimens such as the Maba and Dali specimens, significant differences remain as well. Wolpoff (1980:330) considers the Kow Swamp and Mungo populations to be squarely within the range of modern *Homo sapiens*. This perspective is mirrored by Day (1986:397-400), Klein (1989:396), Larsen and Matter (1985:238), and others. The possibility that the sloping forehead of some of the Kow Swamp specimens (cf., Wolpoff 1980:330; Day 1986; and Klein 1989) may be the result of artificial cranial deformation has further clouded the attempts of researchers to define the taxonomic affinities of this population.

Both Klein (1989:396) and Wolpoff (1980:330) point out that the "robust" Kow Swamp and the more "gracile" (and chronologically earlier) Mungo populations contain ranges of modern to archaic traits, and that these ranges overlap. Finally, a study by Kennedy (1984) seems to indicate that the femora of the Kow Swamp population share close morphometric affinities to modern populations, while contrasting significantly in several features to earlier specimens of archaic *Homo*. In my estimation, the collective weight of these observations seriously casts doubt on the comparative value of the Kow Swamp population to the (apparently) superficial similarities presented by the Lagoa Santa calotte.

Aside from all these considerations, illustrations of the Kow Swamp 1 cranium (e.g., Larsen and Matter 1985:239, 241; and Wolpoff 1980:329) clearly show a series of morphological traits which contrast with the Lagoa Santa calotte. Although the Kow Swamp 1 cranium does indeed show a massive, divided, and arching set of browridges, the cranium is distinctly higher and more rounded in both its sagittal and its transverse outlines than the Lagoa Santa specimen. The Kow Swamp 1 cranium does not appear to have the flattening at the obelion nor the sloping and broadened postero-parietal region which seem to characterize the Lagoa Santa calotte. Additionally, the intersection of the lower frontal bone with the root of the nasal bones in the two specimens show clear contrasts; while the Kow Swamp 1 cranium exhibits a sagittal keel that extends down to the browridge (Wolpoff 1980:330)—a feature which is lacking in the Lagoa Santa calotte.

One possible exception to the perceived problems posed by the morphology, possible cultural deformations, and the relatively late chronology of the Kow Swamp population is the Willandra Lakes Hominid (WLH) 50.

> WLH 50, which is fully sapient in overall morphology and 'Australian' in nature, is very large and robust with marked postorbital constriction and thick vault bones involving enlargement of the diploë... The Calvaria has a flat and receding frontal with a posterior position of minimum frontal breadth, marked temporal crests, a protruding occipital with a well-developed transverse torus, prominent brow-

> ridges, and a maximum parietal breadth located towards the parietal mastoid angle (Habgood 1989:266).

On the basis of electron spin resonance dating, Delson (1985:298) estimates the age of the WLH 50 specimen as somewhere between 30,000 and 60,000 years ago. Habgood (1989:266-67) considers it unlikely that WLH 50 is older than 45,000 years BP; while Habgood (1989:267), Stringer (1989c:76), and Wolpoff (1989a:80) all consider WLH 50 to be morphologically intermediate between the Ngandong specimens of Java and modern native Australian populations.

Turning to Bryan's (1978:321) comparison of the Lagoa Santa calotte to the Maba specimen, we encounter additional similarities and differences. Wolpoff et al. (1984:428) give the following description of the Maba skull cap:

> The specimen possesses some features showing its intermediate position between Sinanthropus and Mongoloids such as a week sagittal keel on the frontal bone, a frontal sinus which is restricted to the interorbital area, the frontonasal and frontomaxillary sutures form a nearly horizontal straight line, the malar surface of the frontosphenoidal process of the zygomatic bone faces more forward than those of Neandertals. Seen from above the supraorbital torus slants posterolaterally, and there is a rather large profile angle of the nasal bone. Given the wide interorbital area the nasal bones are surprisingly narrow (as in Dali), and the nasals support a strongly developed central ridge along their join [sic] (resembling the late Lower Cave cranium H3 as well as the Upper Cave crania). This configuration is still particularly characteristic of some northern Mongoloid populations...

Elsewhere, Wolpoff (1980:253) compares the Maba specimen to the Homo erectus materials from Zhoukoudian, stating that,

> The fairly thick browridges are arched over the orbits (less strongly expressed arching occurs in the Choukoutien specimens). There is a groove between the browridges and the forehead, which bulges centrally very much like the Choukoutien foreheads... Another similarity is found in the rounded shape of the orbits.

On the other hand, Wolpoff points out that that the Maba specimen is "larger than any of the Choukoutien crania," and that

> The vault is broader (especially towards the rear and much thinner. The profile is more evenly curved in both the sagittal and transverse planes. The browridges dip downward over the nose, and there is a high nasal angle and deep depression of the nasal root, suggesting a much more prominent nasal profile (Ibid.).

These last distinctive differences from the Zhoukoudian specimens Wolpoff considers to be grade differences.

At first glance, the Lagoa Santa calotte appears to share many similarities with the Maba specimen. Upon closer examination however, certain important differences emerge. These differences include a somewhat higher cranial vault, decidedly more rounded and discontinuous browridge development, lack of a downward dip separating the browridge from the nose, much more evenly rounded superior orbital margins, and an apparent absence of a sagittal ridge or keel.

Given the highly mineralized condition of the Lagoa Santa calotte, it is unclear as to how many of these differences (especially the absence of a sagittal ridge or keel and the lack of a distinctive dip in the nasal region) are actual or merely products of the fossilization process. The relative cranial height, the postero-parietal flattening and increased breadth, together with the remaining traits described earlier, give an overall impression of a form similar, though somewhat more modern in appearance than the Maba specimen.

Momentarily ignoring the history and location of the Lagoa Santa specimen (that is, speaking strictly on the basis of morphological complex evident from this specimen), one might speculate that had the specimen originated somewhere in northeast China, it would likely be viewed (on morphological grounds) as intermediate between the Maba specimen and the fully-modern Zhoukoudian Upper Cave specimens.

It is important to note that it is not my intent to establish a minimum date of human entry to the Americas on the basis of the Lagoa Santa calotte. Arguing for increased antiquity on morphological grounds is a fundamentally flawed argument on purely logical grounds. We may also observe that morphology usually reflects a range of polygenic and pleiotropic traits which generally obviate systematic attempts at clearly tracing patterns of inheritance. Furthermore, morphological comparisons (like "racial" comparisons) suffer from a lack of consistency in the phenomena used to establish critical distinctions. Nevertheless, the Lagoa Santa calotte (supported by other fragmentary remains from Sumidouro cave), is the only specimen from the Americas which appears to offer a complex of morphological traits which sets it apart from other, fully modern skeletal specimens recovered in the New World. Notwithstanding the acknowledged dangers associated with the use of morphological comparisons, especially when based on such a limited sample, the distinctiveness of these skeletal materials deserves careful if cautious consideration.

If one were to follow the logic behind the argument given by Wenke (above), and in light of the morphological complex exhibited by the Lagoa Santa materials, one could paraphrase his basic chronological argument as follows. Based on the available early skeletal remains, including the Lagoa Santa materials, the initial peopling of the Americas must have occurred after the appearance of comparable Homo sapiens forms in East Asia. On strictly morphological grounds, the Lagoa Santa materials appear to be more modern than the Maba specimen. How much later did the morphological complex seen on the Lagoa Santa calotte develop is unclear. Certainly on the basis of individual traits such as browridge development (rather than on the basis of whole trait complexes), the late date of the Kow Swamp specimens provides a particularly enlightening exception which we may invoke as a cautionary caveat against the use of morphological traits as markers of relative antiquity, and against the use of comparisons based on single traits.

Both the Maba and the Dali specimens have been categorized as examples of Archaic Homo sapiens from the Far East (Pilbeam 1988:131-33; Wolpoff 1980:255). In fact, in one of the early assessments of the Dali specimen, Wolpoff concluded that the Dali and Maba specimens were morphologically nearly identical in their shared clade features (Wolpoff 1980:255). Subsequent descriptions, however, have highlighted some significant distinctions. Klein (1989:247) describes the Dali specimen as follows:

> The skull has a massive supraorbital torus, a low, flat frontal, and an angulated occipital with a prominent transverse torus, all features shared with Homo erectus. The cranial capacity (1,120 cc) does not significantly exceed the H. erectus average, but the parietals are far more expanded than in H. erectus and there is significantly less post-orbital constriction. The face is flat with a canine fossa and without the midfacial projection typical of the Neanderthals.

Additionally Day (1986:386) notes

> The frontal region shows strongly marked vermiculated superciliary ridges, brow ridges that are angulated and not continuous, a distinct frontal keel but no real parasagittal flattening. The mastoid processes are of moderate size. The vault shape from the rear is neither typically tented and erectus-like nor expanded and sapiens-like. The frontozygomatic suture is deeply notched.

Although the general tenor of these descriptions confirms the perceived similarities between the Maba and Dali specimens as suggested by Wolpoff and Pilbeam (above), the observations by Klein and Day also point to greater morphological affinities between Dali and the Zhoukoudian Lower Cave specimens than those shared by Maba and other Far Eastern Homo erectus populations. Furthermore, these distinctions serve to underscore the greater morphological differences which exist between the Dali and Lagoa Santa specimens when compared to the similarities and differences between the Maba specimen and the Lagoa Santa calotte.

Excluding other early southeast Asian specimens such as the Indonesian Homo erectus populations and the Ngandong specimens, the remaining Far Eastern specimens (such as those from Liujiang, and the Zhoukoudian Upper Cave specimens) appear to be relatively recent, fully-modern examples of Homo sapiens. Likewise, to the southeast, coastal-insular and Australian finds (including Niah Cave, Tabon Cave, Wadjak, and Lake Mungo/Willandra Lakes) with dates and estimated ages ranging between 45,000+ years ago and the end of the Pleistocene, all represent various examples of fully-modern, Homo sapiens sapiens populations. On the basis of the morphological traits presented earlier, the Lagoa Santa calotte contrasts markedly from these and other anatomically modern humans. On the basis of these comparisons we may conclude that the morphological traits of the Lagoa Santa calotte and the other fragmentary remains from Sumidouro Cave, Brazil, can best be seen as occupying an intermediate morphological position between the Maba specimen and later, fully-modern populations from the Far East and Southeast Asia.

Beyond these morphological issues, a variety of other issues remain to be addressed. As stated earlier, given the scant and problematic nature of the Brazilian finds, it is difficult to assess whether the traits seen on the Lagoa Santa calotte and other fragmentary specimens from Sumidouro Cave can be generalized beyond a single (or at best a few) specimens to a geographically and temporally distinct population; or whether they represent taphonomic anomalies created by the fossilization process, or the chance preservation of atypical individuals from an otherwise morphologically more modern population. To some extent, similar questions may be posed for the Maba and Dali specimens, although the availability of these later specimens [as well as several more fragmentary specimens from China such as the Xujiayao cranial bones (Wu and Wu 1985:102-104; Klein 1989:247-249), and other more fragmentary remains such as Tongzi, Dingcun, Xindong, and Changyang (Wu and Wu 1985)] for further study, the more secure nature of these finds, and the very existence of several separate specimens from various locations, argues strongly against such considerations.

Another issue of concern hinges on the approaches and assumptions embodied in drawing morphological comparisons. As I have tried to stress earlier in this section, morphological comparisons must be based on trait complexes as a whole, rather than on individual traits (such as the strongly developed browridges) in isolation from other traits. Similarly, every effort must be made to separate genuine genetic traits from ontogenic, cultural, or taphonomic processes which might have altered the traits ascribed to individual specimens. To ignore such possible intervening factors is tantamount to committing a set of fallacies which further weaken or negate the admittedly limited usefulness and validity of any conclusions which might be derived from morphological comparisons (cf. Clark 1978:32-33 and 39-40).

The reporting of such intervening variables is essential if a correct interpretation of morphological traits is to be achieved. Beattie and Bryan's (1984) report points to the presence of postmortem damage producing spurious grooves on the internal surface of the left frontal bone, cracking and warping of a segment of the right posterior parietal area, and the presence of accreted material coating the Lagoa Santa calotte (thus exaggerating the apparent thickness of the calotte and its supraorbital features). These observations all represent potential intervening variables. Fortunately, the stated postmortem damage does not appear to have significantly impacted the gross diagnostic morphological features of the calotte. The accretion of material adhering to the surfaces of the specimen, however, is important, not only because it has added to the thickness of the bones, including the supraorbital features, but because it also has obscured some of the less prominent external features such as the temporal lines, while making impossible a definitive determination of the presence or absence of any weakly developed features which might have characterized the midsagittal region.

The reporting and incorporation of such intervening variables is a matter of routine practice, as for example Day's (1986:386) discussion of the upward crushing of the lower portion of the face of the Dali specimen, or his statement (1986:400) that some of the morphological features of the Kow Swamp specimens may be the result of cultural deformation. In this last example, the excessive emphasis of the browridge development of individual specimens, together with the effects of possible extragenetic factors provide prime examples of the potential pitfalls addressed in earlier parts of this discussion.

Beyond the assessment of morphological factors, the assessment of the chronological significance of the Lagoa Santa specimens requires a consideration of the age of the East Asian specimens to which these New World specimens are compared. The dating of the Far Eastern specimens of Dali, Maba, and Xujiayao, just as the dating of other early skeletal materials from China, has often been viewed as problematic. The traditional approach in the absence of hard chronometric dates has been to use stratigraphic correlations and faunal associations to obtain various age estimates. On this basis Wolpoff et al. (1984:433-34) report that the Dali specimen dates to the "...terminal part of the Middle Pleistocene, 100 ky or more younger than the top of the Zhoukoudian Lower Cave." Presumably on the basis of similar methods of age assessments, Pilbeam (1988:132) suggests that the age of the Dali specimen lies somewhere between 130,000 and 400,000 years ago. Using similar age assessment techniques, Day (1986:386) reports the age of the Dali specimen as late Middle Pleistocene, adding that on the basis of oxygen isotope records of deep sea cores correlated to loessic stages, the Dali site dates between 128,000 to 250,000 years ago.

The Maba specimen's age is reported by Pilbeam as somewhere between 75,000 and 130,000 years ago (Pilbeam 1988:132). Earlier age estimates for this specimen (as reported by Aigner 1978:28) place the age of Maba as "late Riss to mid-Wurm" on the basis of faunal correlations. Citing Woo and Peng (1959), Wolpoff et al. (1984:428) state that the Maba specimen dates "...from the latest part of the Middle Pleistocene or the early part of the Late Pleistocene according to its faunal associations."

Perhaps the most significant improvement in the age assessment of these and other Far Eastern early specimens was reported by Chen and Yuan in 1988. Chronometric dates using Uranium Series (U-S) dating on associated bones have provided additional support for the earlier stratigraphic and faunal age estimates. The ages of the Dali and Maba specimens, also accepted by Klein (1989:247-249), stand at 180,000 to 230,000 years for Dali, and 119,000 to 140,000 years for the Maba specimen; while morphologically similar (Wolpoff 1989a:82) fragmentary remains from Xujiayao have provided uranium series dates between 100,000 and 125,000 years ago (cf. Klein 1989:247; and Chen and Yuan 1988).

Based on the previous morphological comparisons of the Dali, Maba, and the Lagoa Santa specimens, the occurrence of archaic features are most pronounced on the Dali cranium. The Maba specimen exhibits somewhat more modern features, while the Lagoa Santa specimen could be seen as occupying an intermediate position on the basis of its morphology between Maba and anatomically fully modern _H. sapiens sapiens_. Morphologically, the Brazilian specimens also appear to occupy an intermediate position roughly _equivalent_ to that occupied by the Willandra Lakes Hominid 50 (see above, also Delson 1985:298-99; Habgood 1989:266-67; Stringer 1989c:76; and Wolpoff 1989a:80-81). Despite these conclusions, the assessment of the significance of these morphological comparisons relative to the antiquity of human populations in the New World is much more difficult.

Throughout this section I have tried to stress the potential pitfalls and limitations of employing morphological comparisons in order to derive chronological conclusions, even when the intent of this approach is not aimed at establishing a minimum age estimate for initial entry to the Americas. Nevertheless, these observations are presented here as a starting point for use in later discussions. There is no firm justification possible for directly equating the morphological assessment of the Lagoa Santa specimens with specific dates derived from comparable Far Eastern specimens. As a result, we can only return to the argument presented at the beginning of this section to propose some chronological consequences of these comparisons.

_If_ we assume that the morphological characteristics of the Lagoa Santa specimens are representative of the morphological traits of the initial or early populations of the Americas, and that these early American populations represent a sampling of the morphology of coeval or earlier populations in East Asia, then we may conclude that the initial peopling of the Americas probably took place some time after the appearance of the traits which characterize the Maba and Xujiayao specimens in the Far East. This line of reasoning would suggest that the initial peopling of the Americas took place _after_ about 100,000 years ago. Furthermore, if we also incorporate into these morphological comparisons a recognition of the morphologically similar intermediate position occupied by both the Willandra Lakes Hominid 50 and the Lagoa Santa specimens, then we might suggest that it is more likely that the initial peopling of the Americas took place some time after around 50,000 years ago.

As a final note to this discussion it might be worth reiterating the fact that the chronological conclusions reached in this section are intended to represent working hypotheses which are directly dependent upon the current state of our knowledge for both East Asia and the Americas. As such, they represent a necessary first step in establishing a chronological framework which is clearly subject to testing and revision. Aside from the inherent difficulties associated with deriving chronological estimates on the basis of morphological comparisons, the status and significance of the limited Lagoa Santa sample still remains to be established and clarified. If future work should indicate that the data from the Lagoa Santa materials is unreliable, we would be forced to return to the first argument presented in this section which stated that the initial peopling of the Americas must have occurred after the earliest dated appearance of anatomically modern humans in the Far East. The next section approaches this argument by considering the antiquity of Homo sapiens sapiens in the Old World, and particularly near the western Pacific rim.

4.3.3 ADDITIONAL ISSUES, AND AGE ESTIMATES CONCERNING THE ORIGIN OF HOMO SAPIENS SAPIENS

Any attempt to estimate the date of initial entry of native American populations on the basis of data from physical anthropology can not ignore either the data from the Far East, nor the larger issues which have recently dominated scholarly debates about the origins of modern H. sapiens sapiens on a worldwide scale [cf. Durant 1989; Mellars and Stringer 1989; Trinkaus 1989]. The discussions which follow are intended as a review of some of the pertinent lines of inquiry, while recognizing the current, independent, and often disparate interpretations which actively compete and, at other times, support each other in the debate about the origins and antiquity of H. sapiens sapiens. It should be noted that most of these approaches hinge upon data and methodologies which emerged in their own right. These data and methods [with some notable exceptions [e.g., Cann et al. 1987]] have largely ignored or viewed the establishment of the chronology of the New World as a minor issue.

Based on the preceding discussion of the fossil skeletal remains from the Americas and the Far East, the relevance of the "Garden of Eden" or "out-of-Africa" hypothesis championed by Christopher Stringer and others, as well as the multiregional model espoused by Wolpoff and others, are both of prime importance. These two approaches may very well shed some additional light on the possible maximum age of entry into the Americas based on the antiquity of our own species in the Far East. Although many of the critical issues in this debate are largely irrelevant to the main aims of this work, a brief review is offered below because the solution of some of these issues ultimately has an indirect bearing upon the critical issues discussed in this chapter.

Although Wolpoff's [1980, 1989a, 1989b, and Wolpoff et al. 1984] position supporting a multiregional model of human evolution often sounds as a reaction or response to the "out-of-Africa" model espoused by Stringer, the multiregional model is actually based, in part, on earlier works by Hrdlicka [1927], Weidenreich [1938, 1939, 1943, and 1946], Coon [1962], and Brace [1964]. Thanks to the far-ranging influence of Weidenreich's pioneering work on the paleoanthropology of the Far East, many other researchers in the area have incorporated elements of the multilinear model in their interpretation of the evolutionary history of this region.

Ultimately, much of the controversy over the competing multiregional and "out-of-Africa" models can be traced to the contrasting interpretations and classification of European or "Classic" Neandertals, and their relationship to the rise and classification of anatomically modern Homo sapiens sapiens. These differences are not new by any means.

In separate reviews of the "Pre-Neandertal Hypothesis," Bennett

(1979:350-352) and Day (1986:415) trace the major outlines of a position which can be viewed as a precursor to the more recent "out-of-Africa" model to a series of sources beginning in the early 1950s (e.g., Howell 1952). Indeed, Day (1986:415) cites some of the work by Stringer as part of his discussion of the "Preneandertal Hypothesis." I believe that this inclusion is somewhat misleading.

In discussing some of the major outlines of the "Preneandertal Hypothesis," Brose and Wolpoff (1971:1157) state that "one of the earliest hypotheses suggests the sudden replacement of Neandertals by a more modern taxon: anatomically modern _H_. _sapiens_" [in Europe]. They cite Le Gros Clark (1966:116-117), who discusses the "abrupt replacement" of European Neandertals by Aurignacians derived through migration from outside Europe.

In the light of the current components of the "out-of-Africa" model, it is interesting to note that in discussing the "Preneandertal Hypothesis" Bennett (1979:352) concludes with a citation of Birdsell's (1975) views aimed at estimating the degree of genetic admixture and remaining "Neandertal genes" present in modern European populations. I believe that this consideration clearly points to a major distinction between the earlier "Preneandertal Hypothesis" and the more recent "out-of-Africa" model.

Gene flow and genetic admixture were clearly considered a possibility in the "Preneandertal Hypothesis." On the other hand, the modified version represented by the "out-of-Africa" model excludes this possibility. Indeed, Stringer (1990:103) views Neandertals as a separate species (_H_. _neanderthalensis_)—a classification which precludes, by definition, the possibility of gene flow or genetic admixture. At least in this one sense, Stringer's position is far from new. As Klein (1989:266) states,

> In 1864, the Irish anatomist William King suggested that the Neanderthal fossil represented an extinct human species, for which he suggested the name _Homo_ _neanderthalensis_. Most modern specialists follow King to a degree, though they usually place the Neanderthals in an extinct variety or subspecies of _H_. _sapiens_ rather than in a separate species.

Disagreements about the taxonomic status of Neandertal are at the very core of the differences between the Multiregional and the "out-of-Africa" model outputs. These differences (and their consequent impact on the possibility of gene flow between Neandertals and fully modern _Homo_ _sapiens_ _sapiens_ populations) permit the supporters of the "out-of-Africa" model to view some fossil specimens from the Near East (e.g., Skhul, Tabun, Qafzeh) as representatives of a separate species of early anatomically modern humans—distinct from "_H_. _neanderthalensis_"; while supporters of the Multiregional model (cf. Brose and Wolpoff 1971:1181) view such specimens as examples of "a truly transitional group," evolving toward fully modern humans.

In the context of the present discussion it may well be worth noting that the differences in the interpretation of the taxonomic status of European Neandertals is deep-seated and fundamental to these two opposing model outputs. The models are based not only on different interpretations of the significance of fossil specimens at various locations throughout the Old World, but to some extent they rely for their validation on different supporting arguments. The "out-of-Africa" model output relies heavily on the chronological estimates derived from mutation rates of mitochondrial DNA, while the multiregional model output focuses on morphological continuity of local and regional fossils, and to a lesser extent on supporting arguments derived from the archaeological record.

The present review of these two alternative models is not aimed at generating a set of supporting arguments for either model output. Both models present strengths; but more importantly at this point, both model outputs also suffer from a lack of conclusive evidence which could completely falsify the opposing model output. Therefore, it is my perception that the best approach to the incorporation of the evidence presented by these two competing model outputs in the context of the present discussion about their relevance to the timing of the initial peopling of the Americas is to review their impact on the rise of *Homo sapiens* in the Far East (and by extension, to the initial peopling of the Americas), while deferring discussions aimed at evaluating the ultimate validity of either model output.

Perhaps the most important component of the "out-of-Africa" model from the point of view of the present discussion has been the renewed emphasis which has been placed on the chronometric dating of fossil specimens of early anatomically modern humans from the Old World. For over fifty years scattered finds in Africa and the Middle East have produced relatively modern specimens of *Homo sapiens* within contexts which suggested a greater antiquity than the earliest specimens recovered from Western Europe. At the time, most of these finds were dismissed as anomalous. More recently, however, the application of uranium series dating, thermoluminescence, and electron spin resonance dating have been employed to establish the age of specimens which previously were shown to be older than the practical limits of radiocarbon dating, or could only be dated by faunal associations.

At present, fueled in part by the importance of these early representatives of anatomically modern humans to the "out-of-Africa" model output, there is a renewed emphasis on establishing the absolute age of such fossil specimens in many areas of the Old World. As a result, the literature bears witness to a growing number of references to such early modern human specimens.

Early dates reported for African specimens of anatomically modern humans for various sites include dates of 130,000 for Omo-Kibish (Brauer 1984:341; Rightmire 1985:262; Clarke 1985:305; Clark 1988:289); >130,000 for Lake Eyasi (Clark 1988:289); 130,000 to 109,000 for Mumba Rock Shelter (Brauer 1984:342; Clark 1988:289; Klein 1989:288); 120,000 ± 30,000 for (Ngaloba) Laetoli Hominid 18 (Day 1980:55; Day 1986:186,192; Klein 1989:288; Clark 1988:289; Brauer 1984:342); 110,000 to 90,000 for Border Cave (Brauer 1984:344; Clarke 1985:305; Day 1986:330); >100,000 for Florisbad (Clarke 1985:304, 305); 100,000 to 80,000 for Klasies River Mouth (Clarke 1985:305), and 125,000 to 95,000 for these same specimens (Brauer 1984:344); and 70,000 to 60,000 for Porc Epic (Dire Dawa) (Clark 1988:290).

Using dates of 130,000 to 74,000 for the "last interglacial"

(Oxygen-isotope stages 5a through 5e) (Klein 1989:354), Klein assigns this age range to the materials from Border Cave and Klasies River Mouth (1989:288). Using a wider range "between approximately 130,000 and 50,000 to 35,000 years ago" for the early-late-Quaternary (Klein 1989:284), Klein assigns seventeen African sites containing skeletal remains contemporary with European Neandertals (including the Border Cave and Klasies River Mouth sites) to this time period (Klein 1989:285-286). We may note that (excluding some of the more fragmentary remains on this list) the vast majority of these skeletal remains are precisely the same African specimens cited by Stringer (1988; 1989a; 1989b; 1989c; 1990) and Stringer and Andrews (1988) in support of the African origin of anatomically modern *Homo sapiens*.

Outside of Africa, several Middle Eastern sites containing early modern human skeletal materials have also been reported. Principal among these are the sites of Skhul, Tabun, and Qafzeh. Although earlier attempts to obtain radiocarbon dates from Tabun yielded dates between 40,900 and 51,000 years B.P., these dates at the upper limits of radiocarbon dating were considered to represent a minimum age (Day 1986:108-109). Meanwhile, the other two sites had not been dated by radiometric techniques, though they were thought to be roughly contemporaneous with Tabun.

More recently Valladas et al. (1988:616) reported an age of 92,000 ± 5,000 from Qafzeh on the basis of thermoluminescence analysis of associated lithic artifacts. In an article published in this same year, Lewin (1988:1240) points out that this date is in agreement with biostratigraphic data from the site, and with "shortly-to-be-published electron spin resonance results." Furthermore, in a recent article Stringer (1990:102), in discussing the age of the Skhul and Tabun sites, states that:

> The last word has not been said, for the most recent ESR measurements, made in 1988-1989, date mammal teeth from the levels of the human burials at Skhul and Tabun to about 100,000 and 120,000 years ago, respectively.

Assuming that the dating of the African and Middle Eastern specimens is valid, these specimens by themselves clearly bear a tremendous impact on the chronology of anatomically modern humans elsewhere; regardless of one's position about any of the other components of the "out-of-Africa" model output. The independence of these data from the model output's taxonomic placement of Neandertals or from the mitochondrial DNA data must be seriously considered within the context of the present discussion.

As even Stringer (1990:98) has noted, some authors like Erik Trinkaus and Fred H. Smith feel that the volume of genetic and fossil evidence is still insufficient to settle taxonomic issues which require considerations of gene flow, hybridization, and other mechanisms. I tend to agree with this perspective, while I also wish to point out that in the context of this discussion these same competing taxonomic and phyletic model outputs tend to obscure the more relevant impact of the early rise of anatomically modern humans as seen purely from the fossil record.

From my own perspective, and without biasing my conclusions by backing either the "out-of-Africa" model or the multiregional model, I feel that the evidence presented by the fossil record of early anatomically modern humans in Africa and the Middle East has a direct bearing on the possible age range for the initial entry of anatomically modern humans into the New World. It is no longer significant whether one chooses to view such an entry event as the natural extension of the geographic spread of anatomically modern humans out of Africa, or whether one chooses to view the rise of anatomically modern humans in the Far East as a result of relatively coeval developments characteristic of roughly synchronous changes sustained by a laticework of gene flow in a polytypic species over a wide area (Wolpoff et al. 1984:461). The radiometrically-dated rise of anatomically modern humans, at or before 100,000 years ago, clearly fails to restrict the time range of possible human entry into the New World to less than 35 to 40 thousand years as it once did.

4.3.4 HUMAN ORIGINS AND THE CALIBRATION OF GENETIC DATA

On the separate issue of the data derived from the mutation rates of mitochondrial DNA, Wolpoff (1989a:69) objects to the method of calibration when he states

> For instance, Cann et al. (1987:33) determine the base substitution rate of 2%-4% per million years for modern populational divergences from 'known' dates of migration that include 30 000 years ago for the peopling of New Guinea, 40 000 for Australia (a surprising difference since these were the same continent at this time), and 12 000 for the New World. These dates, at the low end of the ranges presented by Stoneking, Bhatia and Wilson (1986), are far too recent even by conservative estimates, perhaps by as much as 50 per cent of the real value. Therefore, in the Cann et al. study the mutation rate has been overestimated, just as it is overestimated in the determination of the chimpanzee-human divergence at 2.1 million years [citations in the original].

Citing independent work by Nei (1985, 1987), Wolpoff (1989a: 69) points out that Nei's work yields a mitochondrial DNA "divergence rate" of 0.71% rather than the 2%-4% value used by Cann et al. (1987). Wolpoff (1989a:70) goes on to present recalculated values for the chimpanzee-human divergence date (based on Nei's figures), indicating that the 6.6 million year value thus obtained is much more nearly in agreement with both the paleontological evidence and the nuclear DNA hybridization data of Sibley and Ahlquist (1984) (6.3 million years) than the 2.1 million year value based on the 2%-4% rate employed by Cann et al. (1987).

Within the framework of the current discussion the most germane observation which must be emphasized is that the data obtained by Cann et al. (1987) incorporate (indeed are calibrated) by means of an assumed date of 12,000 years for the initial human entry into the New World. Whatever value may have been used is inconsequential to the fact that once such a method of calibration has been employed to determine rates of base pair substitutions for mitochondrial DNA per million year units of time, any estimate of the age of initial human entry into the Americas based on these calculations amounts to a tautological trap.

In order to avoid such a trap, calibration (together with _independent tests_ of such calibration) must be derived from alternative sources which do not rely on purportedly 'known' factors about the data base to which these rates are to be applied. I believe the work of Nei cited by Wolpoff, and the recalculated values of the chimpanzee-human divergence dates as discussed earlier, satisfy the requirements which might help us to avoid such a tautological trap. The rates reported by Nei were developed independently, while their confirmation relative to the divergence of the chimpanzee-human lines provides additional independent support of its accuracy. It is interesting to note that when this approach is applied to obtain an estimate of the initial peopling of the Americas, one obtains an age of about 38,000 years ago, rather than the 12,000 years ago value treated as a 'known' by Cann et al. (1987). Although Wolpoff's interests diverge somewhat from the focus of this discussion, it might be noted that Wolpoff (1989a:70) uses this same approach to recalculate the divergence time for modern populations.

A strong argument can be made against the use of archaeological calibration dates, based on relatively short chronologies, such as those employed by Cann et al. (1987:33). Mutations, whether in mitochondrial or nuclear DNA, are products of _random_ errors. In calibration of the _rate_ of random events, shorter periods will be more subject to chance errors than longer periods, in which chance errors tend to be averaged out. This simple fact argues strongly in favor of the use of a 6 million year span as opposed to a 40,000 or a 12,000 year span as the basis from which calibrations should be derived.

For the present, the date for the initial human occupation of the Americas of 38,000 years ago—modeled on Wolpoff's approach using Nei's rate of 0.71%—must be viewed as a tentative first step. Clearly much remains to be clarified before any age estimates based on mitochondrial DNA base pair substitution rates can be applied to resolve chronological issues with a significant level of confidence.

In a recent related study employing genetic distance studies based on blood groups and enzyme polymorphisms, Cavalli-Sforza et al. (1988) utilized 120 alleles grouped into 42 populations to derive a set of phyletic relationships between various modern aboriginal world populations. The phylogenetic tree which resulted from this analysis underwent its first split separating African from non-African populations. The next major divisions of the non-African branch produced a Northeurasian and a Southeast Asian branch. A subsequent series of branchings of the Southeast Asian branch eventually led to various discrete populations including south Chinese, Indonesian, Malaysian, Filipino, Polynesian, Micronesian, Melanesian, New Guinean, and Australian.

The Northeurasian branch split into two branches, a Caucasoid branch and a combined Northeast Asian and American branch. The Caucasoid branch underwent a series of additional branchings leading to various discrete populations including European, Middle Eastern, and Asiatic Indian populations. The other major branch of Northeurasian divided into a Northeast Asian branch (Mongol, Tibetan, Korean, Japanese, Ainu, Turkic, Chukchi, and Eskimo), and an American (Amerindian) branch including South, Central, North, and Northwest Amerindian populations.

Cavalli-Sforza et al. (1988:6004) utilized an assumption of constant evolutionary rates—an assumption which they tested, and which they defend on statistical grounds. Having generated a set of branching relationships based on genetic distance between individual populations and groups of populations clustered into branches, they proceeded to calibrate these genetic distance values utilizing the published date of 92,000 years for the Middle Eastern Qafzeh remains (cf. Valladas et al. 1988), equating this date to the time of the initial split between the African and non-African branches.

Cavalli-Sforza et al. (1988:6004) report a series of genetic distance values for some of the major branches, including 29.7 ± 6.8 for African/non-African; 18.4 ± 3.4 for Australian/Southeast Asian; 16.6 ± 3.5 for Caucasoid/(Northeast Asian + Amerind); 12.1 ± 1.8 for Northeast Asian/Amerind; and 4.2 ± 1.0 for North Amerind/South and Central Amerind. Whereas Table 1 in this report (Cavalli-Sforza et al., 1988:6004) was intended as a demonstration of the constancy of evolutionary rates when the genetic distance values are compared to presumably 'known' archaeological dates of divergence, these same relationships can also be utilized as a tentative source of temporal predictions.

If we assume that the greatest genetic distance value (29.7 ± 6.8 for the African/non-African split) corresponds to the date of 92,000 years ascribed to the Qafzeh specimen (Valladas et al. 1988); and if we assume (as Cavalli-Sforza et al., 1988 claim) that genetic differentiation or evolutionary rates accumulated at a constant rate, then we may calculate on this basis a set of projected dates corresponding to the times when some of the major branches split from each other. Table 4.1 shows some relevant calculations.

These calculations lead to a projected age of 56,997 ± 10,532 years for the split between the Australian/Southeast Asian branches, and a projected age of 37,481 ± 5,576 years for the split between the Northeast Asian/Amerind branches. Interestingly, this last figure is very near the 38,000 year figure derived earlier based on the recalibration of the mitochondrial DNA ages as based on the work of Nei and Wolpoff (see above).

One may note, however, that Stringer (1990:102) reports a new set of dates (100,000) for Skhul, and (120,000) for Tabun (see also Stringer et al., 1989:757). If we recalibrate the genetic distance values presented by Cavalli-Sforza et al. (1988) on the basis of each of these other dates, we obtain the following alternative values.

Based on an African/non-African split date of 100,000 years we get an Australian/Southeast Asian projected date of split of 61,953 ± 11,448 years, and a Northeast Asian/Amerind projected date of divergence of 40,741 ± 6,061 years. Based on an African/non-African divergence date of 120,000 years, we get an Australian/ Southeast Asian projected date of divergence of 74,343 ± 13,737 years, and a Northeast Asian/Amerind projected date of divergence of 48,889 ± 7,273 years. Given differences in the age of referents of calibrations together with plus-and-minus factors, the average date for the separation of Northeast Asian/ Amerind branches falls around 44,000 years ago, with a potential range of 56,000 to 32,000 years ago. The significance of these and other chronological data presented in this chapter will be discussed further in section 4.3.10.

Table 4.1

Chronometric Calibration of Genetic Distance Values

Population Branchings	Genetic Distance	Time
African/non-African	29.7 ± 6.8	92,000* 100,000** 120,000***
Australian/S.E. Asian	18.4 ± 3.4	56,997 ± 10,532* 61,953 ± 11,448** 74,343 ± 13,737***
N.E.Asian/Amerindian	12.1 ± 1.8	37,481 ± 5,576* 40,741 ± 6,061** 48,889 ± 7,273***

Note: This table is based on the data provided in Cavalli-Sforza et al., (1988). Genetic distance values (above) are calibrated on the basis of dated early *Homo sapiens sapiens* specimens outside Africa. The actual date (e.g., 92,000 years BP) is divided by the African/ non-African genetic distance value (29.7) to obtain a specific multiplier value—in this case the multiplier is 3097.6431.

*Calibration value based on Qafzeh (Valladas et al., 1988:616) - multiplier 3097.6431 x genetic distance.

**Calibration value based on Skhul (Stringer 1990:102; Stringer et al., 1989:757) - multiplier 3367.0034 x genetic distance.

***Calibration value based on Tabun (Stringer 1990:102) - multiplier 4040.4040 x genetic distance.

4.3.5 DENTAL CHRONOLOGY

In many respects the problems which surround the acceptance of the chronological projections offered by Cann et al. (1987) resurface in our consideration of the work of Christy Turner (cf. 1983, 1985, 1986a, 1986b, 1987, 1989, 1990; and Nichol and Turner 1986). Working with dental morphological traits, Turner has amassed an impressive array of statistical evidence, based on the frequency of various dental morphological traits, which has allowed him to segregate circum-Pacific populations on the basis of multivariate mean measures of divergence (MMD's).

Turner's conclusions regarding the composition and sequence of branches of populations (with minor exceptions) closely mirrors the genetic distance-based branchings obtained by Cavalli-Sforza et al. (1988). Two major groups (Sundadonty and Sinodonty) equate reasonably well with the Southeast Asian and Northeurasian branches of the Cavalli-Sforza et al. (1988:6003) genetic tree, although small differences (like the classification of the Ainu) remain, while Turner's classification does not address the inclusion of the Caucasoid group under Sinodonty (the equivalent branch of Northeurasian in the genetic tree—also compare to the results of dental trait analysis obtained by Hanihara 1979). Additional similarities in Turner's analysis also point to discrete differences between Eskimo/Aleuts, Northwestern North American ("Na-Dene"), and Paleo-Indians (the remaining populations of native North, Central, and South Americans).

Turner attributes the development of these population differences to a variety of diachronic factors such as "low level selection" (Turner 1985:48) for the development of the Sinodont pattern (also Turner 1987:311); but "the pattern of modern and recent worldwide dental variation as presently known doesn't make much sense if viewed as responses to local evolutionary forces" (Turner 1987:317); microevolution (Turner 1986b:1140 and 1985:48) of stable polygenic traits which "vary minimally in response to environmental influences" (Turner 1989:88); and genetic drift, "bottlenecking," and clinal distributions (Turner 1985:48). Although Turner has published his data and methods widely (see above) these publications have not addressed in systematic detail issues relative to specific processes which could account for populational differences in dental morphology.

The following critique of turner's work does not ignore the strengths of his approach. Turner has amassed an impressive data base. His approach is ingenious, well-documented, inductively defensible, and mathematically sound. My concerns about his work do not relate to any of these aspects, but rather to some of the underlying assumptions and methodological problems; and the defensibility of some of his conclusions, particularly regarding his chronological projections.

In assessing Turner's approach we may note that if dental traits have been changing at a steady rate within the time and area which form the focus of his work, then this sample should be viewed as a spatio-temporally limited sample of dental trait shifts drawn from a much larger universe covering a much wider area and spanning a much longer time period. Therefore, the validity of the assumption of steady rates of change in dental traits should be tested against a much larger sample derived from a wider geographical area, and covering a longer span of time. Even in the absence of clear demonstrations of the mechanisms which account for the observed patterns of dental variation, an essential first step toward defending the use of these patterns as a source of valid data for the reconstruction of the biohistory of populations demands the presentation of data and analyses from comparable, independent, or non-overlapping samples. Without such an approach, it is difficult to evaluate empirically the validity of his assumptions, the significance of the patterns which he presents, or the validity of the biohistorical reconstructions which he proposes for circum-Pacific populations.

Although Turner mentions various biological mechanisms which *might* account for the statistical patterns which he documents, it is difficult to address the significance of the observed patterns in the absence of detailed and comprehensive mechanisms (both in general and in the case of specific populations and traits) without having to make assumptions about such mechanisms not formally detailed in Turner's publications. The requirement that conclusions about empirical observations should be based on internally consistent explanations of mechanisms, and that they should be explicitly presented and tested, is fundamental to all science.

Lacking an explicit framework detailing the proposed mechanisms which might account for the observed patterns of variations in dental morphology, the following comments and observations should not be viewed as a critique of factors which may or may not have been incorporated into the formulation of Turner's model; but rather, they should be viewed as tentative initial concerns which may be clarified once a comprehensive and detailed discussion of these and other related issues is published by Turner.

Turner utilizes a combination of dental traits which includes both discontinuous variables (minimally presence or absence of certain traits) together with a ranked set of values (zero to seven) aimed at categorizing various degrees of expression of a variety of continuous variables. Size and degree of development observed for traits showing a continuous range are fundamental components of Turner's approach.

Whereas Turner acknowledges the polygenic nature of dental traits (Turner 1985:33, 1986a:37, and 1989:89), he also argues against the significance of sexual dimorphism or the likelihood that separate phenotypic traits may be controlled by a single gene or gene complex (i.e., a pleiotropic set of relationships) (Turner 1985:33). In this context, the published results of Alvesalo and Kari (1977), Alvesalo and Varrela (1980), Alvesalo and Portin (1980), and Alvesalo and Tamissalo (1981) strongly argue against these last two conclusions, while presenting a potential source of additional intervening variables to the values derived from the observation of continuous variables. In this light, it seems that careful segregation of dental samples by sex must *precede* any calculation of the frequency and expression of dental trait variants *before* using these values to characterize populations. Even the assumption of approximately even sex ratios in the original populations (hardly a reliable assumption based on modern cross-cultural observations) can not be automatically transferred to fossil assemblages whose composition may be skewed due to various cultural and taphonomic factors.

Turner argues (from my own view, quite correctly) that "acquired traits, however, are subject to much more environmental influence than those that are genetically inherited" (Turner 1989:88). At the same time we may note that observations of dental traits are based on phenotypes from which genetic traits are inferred. Differential phenotypic development and morphogenesis can obscure the genetic component of phenotypically-observed traits, introducing a source of potential error which can not easily be accounted or controlled.

Beyond these fundamental concerns, we must address certain subtle methodological issues. One of the more significant methodological issues relating to the consistent observation of dental traits was addressed by Nichol and Turner (1986). In this study Turner and his student (Nichol) examined 300 casts and a nested sample of 50 casts several times over several months. Degrees of intra- and inter-observer concordance were quantified and reported in this article.

Combined scores of inter- and intra-observer variability yielded concordance values higher than 90% for only nine out of 24 traits (37.5% of the traits). Concordance levels for the remaining 15 traits (62.5%) ranged in value from 32% to 89%. Within-observer (intra-observer) concordance rose with subsequent observations of the same sample, indicating that familiarity and learning were a component in concordance.

The larger number of traits (62.5%) with concordance values below 90% —despite the fact that one observer was trained by the other observer—raises major questions about replicability of results. The fact that learning through repeated study of the same sample improved concordance, raises questions about accuracy and the quality of single as opposed to multiple observations which are mixed or compared in published samples which do not report this dimension of the observations. Finally, given the reported variability between observers and intra-observational results, the statistical characterization of extinct and living populations on the basis of these traits potentially introduces a source of errors which are of unknown value; and which may obscure, exaggerate, or negate statistically significant differences between populations.

Having expressed some of the concerns inherent to Turner's methodology as reported in the literature, we may proceed to consider his use of these methods to offer chronological projections of biohistorical events of relevance to the initial peopling of the Americas. Using an admittedly large data base in order to achieve a high level of statistical significance (90 to 95%) (Turner 1986a:37), Turner (1985; 1986b) assumes a slow, steady rate of (genetic?) change expressed as shifts in dental morphology which he calibrates on the basis of assumed dates of separation of ancient Northeast Asian populations from archaeological Aleutian populations; and to a lesser extent, separation from Paleo-Indian populations (Turner 1985:37-41; 1986b:1140). On this basis Turner develops a ratio of mean measures of divergence (MMDs) to 1,000 year units of time which he uses to project dates for various biohistorical events.

Turner (1983:157 note 2) states that

> The marked difference between Aleut-Eskimo and Indian suggest more time for their divergence than can be demonstrated for the occupation of the New World. However, the substantial difference between Aleut and Eskimo could mean that they too began to diverge in Siberia rather than in Alaska.

Certainly, this is a reasonable hypothesis, just as it would be equally reasonable (assuming that his approach to chronological projections is valid) that the explanation for the observed marked differences is due to an incomplete or incorrect understanding of the relevant archaeological record. Demonstrating a bias which can not be defended on the basis of the dental evidence, Turner does not address this alternative hypothesis. Turner's claim that the earliest populations of the New World separated from the Northeast Asian populations around 14,000 years ago (Turner 1986b:1140), resulting in population movements which ultimately permitted the arrival of Paleo-Indians south of glaciated Canada sometime around 12,000 years ago (Turner 1986a:44; 1986b:1140; 1989:94) must be evaluated in the light of his bias against accepting the possibility that his own data might validly point towards the existence of a problem with archaeologically derived, and contradictory chronological projections.

In order to accept the validity of Turner's chronological projections, several points must first be demonstrated. These include, but are not limited to, demonstrations that a) the measures of dental variation presented by Turner are statistically valid to the exclusion of observational errors (such as the ones which were responsible for the inter- and intra-observer concordance differences); b) the archaeological dates used to establish the time(s) of inter-group populational separations are correct and represent the earliest demonstrable dates available; c) the rate of phenotypic change in dental traits is a function of the rate of mutation alone (independent of genetic drift, natural selection, etc., which may have changed gene frequencies at independent rates from rates of change due to mutation alone); d) the rate of mutation is frequent enough and constant enough to produce a steady rate of change which is the same in all populations; and e) all the mutations will have an equal phenotypic effect (expressed as an equal degree of statistical difference in dental traits —a proposition which ignores the potentially vastly different phenotypic effects of mutations in structural as opposed to regulatory genes, as well as the difference between monogenic, polygenic, and pleiotropic effects).

Unfortunately, as I have tried to indicate earlier, many of these assumptions remain undemonstrated. Furthermore, as in the earlier case involving Cann et al.'s (1987) use of archaeologically-derived dates from the Americas to calibrate rates of mitochondrial DNA changes, such calibration precludes the dating of the initial entry of populations into the Americas because it commits a tautological error of the first magnitude. As a result of similar problems, the validity of Turner's use of shifts in dental traits as a valid basis for calculating the age of entry of man into the Americas should also be rejected.

4.3.6 THE INITIAL ENTRY POPULATION: INFERENCES REGARDING MIGRATIONS AND POPULATION DYNAMICS

In an earlier work, T. D. Stewart (1973:7) characterized the geography of the Americas as a "biological cul-de-sac." The most fundamental outcome of this perspective points to the fact that once human groups had entered through what is now Alaska, the uninhabited frontier lay ahead of them, always largely southward, in the direction of Tierra del Fuego. The significance of these observations are most pertinent in relation to any attempt to assess the geographical distribution of biologically distinctive populations.

In a recent set of publications, various authors [cf. Greenberg et al. 1986; Greenberg 1987; Williams et al. 1985; Cavalli-Sforza et al. 1988; Turner 1989; and Schurr et al. 1990] using independent lines of evidence, have proposed that the Americas were peopled by three distinct migration events yielding three biologically and linguistically distinct descendant populations (Eskimo-Aleut, Na-Dene, and Amerind [all other Native American groups]). In light of the geographical orientation of the Americas relative to the point of entry of prehistoric populations, and in light of the entry and movements discussed above, it is likely that such a sequence of migration events would have resulted in a predictable set of geographical distributions based on the sequence in which such groups entered the Americas. The earliest populations would have occupied the most distant portions of the Americas from the point of entry, while those entering last, would have settled closest to the point of entry.

The observed geographical distribution of traits in modern populations which enabled researchers to postulate three distinct migration events suggests a separation of the initial (Amerind) and the other two populations by a significantly long interval of time. The period of time between the arrival of the first and second groups appears to have lasted for a sufficiently long period to allow the initial population to spread and establish itself in sufficient numbers to significantly resist displacement by subsequent populations. The temporal hiatus suggested by the geographical location and unequal areal distribution of their modern descendant populations could either be the result of separate migration events, or the influence of isolation due to glacial barriers during the Wisconsinan.

In this context, due to the land bridge which joined Alaska to Asia, and the presence of glacial ice to the east and south, Alaska can be viewed as part of Asia while the habitable portions of the New World lay south of the ice sheets. In view of these geographic factors, "entry into the Americas" means different things during and after the Wisconsinan. These distinctions, in turn, may further complicate communication about the number of actual past migrations; and the original, interim, and eventual geographic homelands of subsequent groups. While such distinctions are very important in a broad sense, the selective focus of this work on the earliest initial entry population precludes the need to discuss these distinctions in greater detail at this point.

Given the cul-de-sac configuration of the Americas relative to entry through Alaska, we may also propose two other observations. First, it can be pointed out that the areas most distant from the point of entry would have been settled by the descendants of the initial entry population. This conclusion is in keeping with the temporal priority of initial entry, as well as the realization that the advantages associated with decreased competition from neighboring groups associated with the presence of an uninhabited frontier zone would have been most directly applicable to the spread of such a population. The modern Na-Dene and Eskimo-Aleut groups occupy, in large part, areas which must have been settled only after the glacial ice had melted.

Second, although it is impossible to quantify the relationships which might have developed between the geographical area settled by the initial Amerind group and the time interval between the first and subsequent entry events, it is not unreasonable to assume that settlement of large contiguous geographical areas by a genetically distinct and homogeneous population capable of resisting incursions and geographical fragmentation by subsequent populations must have required the passage of significant periods of time. Thus, in a broad sense, the larger the contiguous and unfragmented geographical area occupied by the descendants of the initial entry population, the greater the interval of time which must have elapsed between the first and proposed subsequent entry events.

In light of the above arguments, it is especially significant that the "three migration model" for the peopling of the Americas yields a geographically very uneven subdivision of Native American populations. Given the uneven nature of these subdivisions, it is reasonable to assume that there must have been a substantial temporal hiatus between the end of the initial entry event and the onset of any subsequent entry events. At the same time, we must also acknowledge that, by themselves, these observations can not be made to yield any specific quantitative estimate of the length of time represented by this temporal hiatus.

As noted earlier, both the morphological and genetic composition of Native American populations support the conclusion that the Americas were peopled by early populations which originated in Northeast Asia. In this regard, the analysis of the population dynamics of this region during the Pleistocene presented by Wolpoff et al. (1984) presents a particularly pertinent model for the assessment of populational events which preceded and resulted in the peopling of the New World.

Drawing on a variety of sources of interpretation, including population genetics, paleoanthropology, and cultural theory, Wolpoff et al. (1984:450) propose a theoretical construct involving a sequence of population movements, segmentation, and isolation which would have produced a succession of founder effects yielding small, genetically discrete populations. The gene pools of these populations could be characterized as containing subsets of the total genetic diversity of types and frequencies which characterized the larger contiguous parental populations at more southerly temperate latitudes. This construct, labeled the "center and edge" hypothesis, presents an explicit and defensible mechanism which can account for the origin of discrete and distinctive populations which may have entered the Americas during several separate entry events.

The genetic data of Williams et al. (1985) and Schurr et al. (1990) provide direct corroboration which support the validity of the "center and edge" hypothesis in relation to the genetic composition and relationships between Native American populations and East Asian populations, and the biohistorical events postulated by Wolpoff et al. (1984). These correspondences between the genetic data and the biocultural mechanisms posited by the "center and edge" hypothesis also account well for the multiple entry events postulated by Greenberg et al. (1986), Greenberg (1987), Williams et al. (1985), Cavalli-Sforza et al. (1988), Turner (1989), and Schurr et al. (1990).

4.3.7 THE INITIAL ENTRY POPULATION: CONSIDERATION OF GENETIC DATA

Implicit in the data and conclusions of Williams et al. (1985), Cavalli-Sforza et al. (1988), and Schurr et al. (1990) lies the perspective that Native American populations represent partial genetic subsets of the total observed traits and genetic frequencies seen in modern East Asian populations. This perspective is strengthened by the realization that each set of researchers employed a different data base (Williams et al. 1985 used distributions of Gm haplotypes; Cavalli-Sforza et al. 1988 used 20 alleles from five major blood group systems; and Schurr et al. 1990 focused on maternal lineages of mitochondrial DNA).

Another way to phrase these relationships diachronically would be to state that genes which are polymorphic in East Asian populations underwent changes in gene frequencies in their descendant American populations, a process which resulted in some cases in the loss of one or another allele, producing monomorphic populations in the New World. This is precisely the genetic signature predicted by Wolpoff et al. (1984:452) which would result from the dynamics associated with the "center and edge" hypothesis—a set of mechanisms which can be ascribed to a general class of phenomena known in population genetics as sampling error.

The observed differences in genetic composition and gene frequencies between East Asian and Native American populations (when viewed singly, one allele at a time) could theoretically be explained by alternative mechanisms. Any one allele of a gene present in East Asian populations but absent in New World populations may have entered the gene pool of East Asian populations through mutation or gene flow from populations to the west after the ancestral New World populations had become isolated from the larger East Asian gene pools.

In assessing the viability of these alternative explanations, one must take into account the number of such individual genetic variations which separate East Asian and New World populations, as well as the relative gene frequency values of individual alleles in East Asian populations. For example, it is highly unlikely that alleles introduced through mutations or gene flow to a large standing or stable population would spread and reach significant gene frequencies, even over extended periods of time, in the absence of pronounced selective pressures.

Perhaps more importantly, the probability of the occurrence of such events over multiple and separate gene systems will rapidly decrease as the product of the probability of each additional gene system is factored into the overall probability. The observed recurrence of subset differences in a wide range of gene systems in Native American populations when compared to East Asian populations renders this alternative interpretation of the origin of the observed genetic differences highly unlikely.

Despite this conclusion, the arguments presented against an explanation of genetic differences between East Asian and Native American gene frequencies, when based on any one specific gene system, can not be conclusively ruled out. Thus, when examining such differences on the basis of a single gene system, we can still propose two principal alternative hypotheses.

1) The observed differences may have been the result of the appearance of a specific allele (absent in Native American populations) in East Asian populations after the separation and isolation of the ancestral American population from its parental East Asian population.

2) The observed absence of an allele in Native American populations (but present in East Asian populations) may have resulted from the loss of that allele due to:

a) the effects of natural selection; or
b) the result of sampling error.

These hypotheses are addressed here in relation to the published data on various genetic systems for New World and East Asian populations. In addition, several other pertinent considerations or preconditions guided the approach toward data selection and gathering.

1) Early in the process of assessing the potential implications of each of these alternative hypotheses, it was noted that the "center and edge" hypothesis (as compared to the results of several broad-based independent genetic studies) appeared to be a valid and defensible model in the study of the biohistorical processes which gave rise to the various Native American populations. Additionally, it was noted that if a gene system could be found in which contrasting gene frequencies could be demonstrated (as a result of the systematic elimination of alternative hypotheses) to have been the result of sampling error, mathematical modeling techniques were available which might yield additional biohistorical insights.

2) A contrastive frequency of an allele in a well-established gene system whose gene frequency reaches significant values in other populations worldwide was deemed to be least likely to have arisen as a result of mutation or gene flow at a late date in human evolution—particularly, if the gene frequency of its various alleles had high values in East Asian populations.

3) Since sampling error is a random process, it would be unreasonable to expect that all or even a significant proportion of gene systems would show an equivalent level of contrasts in their gene frequencies. Some would show major differences, while others would essentially be non-contrastive. Preference would be given to highly contrastive gene systems, independent of any bias introduced by the realization that other gene systems might not show the same level of contrasts in their gene frequencies.

4) Given the temporal priority of entry by the ancestors of Native American populations whose descendants are geographically most distant from the point of entry into the New World, the most reasonable sources of genetically contrasting data detailing the biohistory of the first entry event would be drawn from South American Native populations.

5) For the purposes of clear quantitative studies which could also be evaluated with a fair degree of certainty in relation to the hypotheses proposed earlier, preference would have to be given to mono-

genic traits whose inheritance and phenotypic expression could be documented with high levels of confidence.

6) Finally, in the interest of practicality, well-known traits which had been studied and well documented worldwide over many years, and for which there exists a large and detailed body of published data in the literature, would be preferred. This last precaution was intended to eliminate spurious conclusions drawn from chance skewing introduced by unrepresentative data or poorly known genetic mechanisms.

In order to test the two principal set of hypotheses proposed above, an initial survey of the genetic literature found in standard journals was conducted. This survey was limited to various primary sources of genetic data. The results of this survey are summarized in Table 1 of Appendix 1. Following this survey, other sources, such as Mourant et al. (1976) and Roychoudhury and Nei (1988), which provide worldwide summaries of genetic data were also consulted.

In light of the five preconditions stated earlier, morphological traits (e.g., stature, and other anthropometric data) were eliminated from consideration early during this survey. Morphological traits whose variant forms had to be deduced from their phenotypic expression could have easily been influenced by environmental, ontogenic, cultural, and other such factors, which would introduce uncontrolled variables that would be difficult to evaluate or eliminate. Furthermore, it was felt that many morphological traits would likely be the products of polygenic or pleiotropic patterns of phenotypic expression which would be nearly impossible to clarify sufficiently to produce an unequivocal and reliable set of results.

During this initial survey another large group of genetic systems were also eliminated from consideration because they were either poorly contrastive or non-contrastive, or because there were not sufficient data from a worldwide perspective. Still others were eliminated because they had only recently come under detailed study; and thus either their mechanisms of inheritance or expression were not clearly documented, or their worldwide distributions were poorly documented, or both.

A major component of the data uncovered during this early survey documented the nature and distribution of various blood group systems. Among these, by far the most promising blood group system fitting all the preconditions of this study was the ABO blood group system. This blood group system had been thoroughly studied worldwide, its mechanisms of inheritance and expression were well known, its worldwide distribution had been thoroughly sampled and reported, its expression was clearly monogenic, the frequencies of its alleles were almost universally polymorphic with significantly large gene frequencies, and it showed clear contrastive values between Native South American populations and East Asian populations. On the basis of these observations a second, more detailed survey was conducted that focused on the gene frequency distributions of the ABO blood group system in Native American populations, drawn from primary sources. The results of this second survey are summarized in Table 2 of Appendix 1. Secondary sources were also subsequently consulted. They are cited at the end of Table 2.

The primary sources cited in Table 2 of Appendix 1 cover 147 North, Central, and South American groups encompassing a total of 17,481 individuals. Wherever possible, preference for inclusion in this sample was given to groups which the authors deemed to be without non-Native American admixture. Estimations of admixture in this body of literature ranged from historical summaries to sophisticated statistical tests and comparisons to local mixed and non-Native American populations.

A set of symbols reflecting the evaluation of the authors of these studies of the relative degree of estimated admixture in each group is also reported in Table 2. A comparative sample of ABO gene frequency values of Spanish (Caucasoid) populations is also reported at the end of Table 2. Finally, ABO gene frequency values for a large sample of Chukchi and a smaller Northeast Siberian Eskimo populations are given for comparative purposes. ABO gene frequencies for other East Asian populations were derived from the data provided by Roychoudhury and Nei (1988), and by Mourant et al. (1976).

Appendix 1, Table 2 also cites two additional sources which summarize ABO gene frequency distributions from North and South American native populations. Spuhler (1972) provides a survey of 21 North American populations, while Salzano (1957) reports ABO gene frequency values for 58 South American populations. The data presented in these two surveys are largely non-overlapping with the survey of primary sources presented in Table 2. The gene frequencies in these two studies were also included for consideration in the discussions which follow below, but were not reported by individual groups in Table 2.

From a broader perspective it may be noted that the ABO blood group system has been known longer than any other blood group system. The results of the surveys summarized in Appendix 1 agree with the other broad surveys cited above.

Mourant et al. (1976:4-7) provide a concise discussion of some of the major features of the ABO blood group system. These authors indicate that millions of individuals have been tested for this blood group system, with samples derived from practically every country throughout the world. The combined estimated gene frequencies cited by Mourant et al. (1976:6) for the world are .623 for O; .215 for A; and .162 for B. The vast majority of world populations show a polymorphic distribution of gene frequencies for this blood group, with the notable exception of the South and Central American Indians who, even after five hundred years of contact with non-native populations, still show almost exclusively type O blood (gene frequencies of 1.00 for type O). The authors whose work is presented in Appendix 1, Table 2 agree with the conclusion of Mourant et al. (1976:6) in stating that these figures reflect the complete original absence of alleles A and B from the gene pools of these populations prior to 1492 A.D. The allele for type O blood is present with gene frequency values in excess of fifty percent in all populations worldwide.

The allele for type A blood is found worldwide as well, with typical frequencies ranging from slightly over ten percent to somewhat over forty percent. Relatively high gene frequency values for this allele (around 30 percent) are found in Europe and Australia. Interestingly, the extremes in high and low values are both found in the

Americas, the low value from South America, while the high value (over 50 percent) comes from North American Blackfoot Indian populations. The significance of the high gene frequency value of type A blood among the Blackfoot Indian populations will be discussed below.

The allele for type B blood has gene frequency values which range from a low of over five percent in Western Europe, values ranging between ten and twenty percent in Africa, to nearly thirty percent in Asia. Australia and South America are both conspicuous by their total absence of type B blood, while gene frequencies for type B blood in North America are extremely low generally, with the notable exception of the native Arctic populations. On a worldwide basis the highest values in the gene frequency of type B blood occur in Asia, particularly in Central Asia. East Asian populations show gene frequency values for type B blood which range from fifteen to twenty-five percent, with a typical value around twenty percent for most populations.

From a purely biohistorical point of view, the distribution and variations in the gene frequencies of these alleles deserves attention and explanation in light of the hypotheses proposed earlier for single gene systems. Among the first issues which need to be addressed is the origin and antiquity of the ABO gene system and its component alleles.

As indicated earlier, the continuous and high frequency of the allele for type O blood on a worldwide basis is essentially noncontrastive. In a sense, the presence of this allele seems to be a fundamental trait associated with modern *Homo sapiens* everywhere. Somewhat similarly (with the exception of Native South America, and perhaps Central America) the distribution of the allele for type A blood (though in lower frequencies) is similarly ubiquitous. Finally, with the exceptions of Australia and Central and South America, the allele for type B blood also has a worldwide distribution. On this basis, collectively, it seems highly unlikely that the ABO blood system in *Homo sapiens* represents a new genetic development.

Comparisons of the blood chemistry of modern humans and various non-human primates have produced surprisingly large numbers of correspondences, including the presence of various blood group systems first isolated in human populations. Although the observed differences in the details of these various blood group systems between human and non-human primates and among various non-human primates appear to increase as comparisons are made between more distantly related species, equivalents to the ABO, MN, Rh, Xg, and secretor systems have all been identified in non-human primates by the use of antisera developed for use with human samples (see Wiener and Gordon 1960; Wiener and Moor-Jankowski 1963; Wiener et al. 1963; Moor-Jankowski et al. 1964; Moor-Jankowski and Wiener 1965; Buettner-Janusch 1966; and Reynolds 1967).

The significance of these findings is fundamental to an understanding of the antiquity of the ABO blood system in human populations. The presence of these equivalences in blood chemistry between human and non-human primates strongly suggests that these gene systems represent plesiomorphic traits developed long before the phyletic divergence which ultimately led to our own species. In this view, the increasing differences which accumulate in the details of these blood group systems as comparisons are drawn between more distantly related species further confirms this hypothesis. Such differences are precisely what might be expected as derived or apomorphic variants continue to accumulate over increasing periods of genetic isolation. In the words of Buettner-Janusch (1966:521),

> Since we now know that the great apes, like man, are polymorphic for these [ABO] antigens, it is simpler to suppose that the ABO blood groups are part of man's primate heritage. Furthermore it does not seem reasonable to argue any longer over whether blood group A or O was the original human ABO blood group and whether group B arose from a mutation after the phyletic branching of Homo. Since the B antigen has been found among orangutans, gibbons, siamangs, and gorillas, it too appears to be part of the gene pool which man inherited.

Therefore on this basis [i.e., an expression of Dollo's law (Hickman et al. 1979:11)], I believe that we are well justified in ruling out the possibility that the absence of the allele for type B blood in the gene pools of Native South American populations is due to the separation of the ancestors of these populations from East Asian populations prior to the rise or introduction of this allele in their common ancestral population. The relatively high frequency values of this allele in modern Asian and East Asian populations when compared to frequencies in other populations worldwide only serves to strengthen this conclusion. Therefore, other explanations must be examined to account for the contrasts in the gene frequency of blood type B between modern East Asian and Native South American populations.

The emphasis on type B blood in this discussion, over and above the importance of the absence of type A blood in the gene pools of Native South American populations, needs to be clarified at this point. The data on gene frequencies summarized in Appendix 1, Table 2, as well as the data on these frequencies obtained from other published sources, are derived from populations in which admixture from immigrant Caucasoid and Negroid populations over the last five hundred years can not be categorically ruled out. Given the relative frequencies of each of the three alleles in the original Caucasoid and Negroid populations, admixture and less direct gene flow probably account for the source of the occasional and very low frequencies of type A, and the even less common type B alleles encountered among relatively isolated Native South American populations.

Primary sources which provide detailed evaluations of the source and manner of admixture responsible for the incidence of alleles other than type O often provide solid historical, genealogical, or statistical data which clearly accounts for these minor components. Nevertheless the possibility exists, however remote, that a very small amount (indeed, possibly gene frequency values under one percent) of type A blood may have been present in the gene pools of pre-contact Native South American populations. This view has been fueled in part by the publication of results of analysis of highly questionable blood group data derived from mummified remains (e.g., Llop and Rothhammer 1988).

It is doubtful that this issue will be satisfactorily and conclusively resolved any time in the near future, if ever. Furthermore, in any event, the gene frequency of blood types A and B in prehistoric Native South American populations are highly unlikely to have had values significantly over .01, if they were present at all—while the contrast to the East Asian gene frequency values for these alleles is evident. Therefore, although the possibility that these alleles might have been present in exceedingly low frequencies among prehistoric Native South American populations can not be categorically ruled out, I have opted to ignore this possibility in the discussions which follow until more reliable data and arguments are presented in support of their potential presence among these populations.

One reasonable alternative hypothesis (formally introduced at the beginning of this section) which might account for the observed differences in gene frequency distributions for type B blood between modern East Asian and Native South American populations involves the mechanism of natural selection; namely, some specific environmental factor or combination of factors might have systematically decreased the fitness of individuals with type B blood, resulting in the loss of this allele from the gene pool of Native South American populations. Interestingly, the noted simultaneous absence of type A blood in these populations requires potentially similar explanation.

Invoking natural selection, however, is only the first step. The effects of natural selection on the gene frequencies of a gene system in a given population can be classified into four categories: normalizing selection; diversifying selection; balancing selection; and directional selection (cf. Ayala and Valentine 1979:125–143). Each of these mechanisms results in different outcomes over time.

Normalizing selection reduces variance in the phenotypic expression of a trait. This type of selection often results in genetic homeostasis wherein the gene frequencies of a population will tend to return to their former values after the incidence of some temporary changes in gene frequencies. The highly contrastive gene frequencies of type B blood discussed above do not seem to suggest this type of selective pattern.

Diversifying selection maintains genetic polymorphisms when separate alleles in some type of organism experience different fitness values in different localized conditions in a patchy environment. Polymorphic diversity in the gene pools of such species is therefore a reflection of the diversity present in the natural environment occupied by different local populations. Diversifying selection can only maintain genetic polymorphisms in local populations so long as continuous or repeated gene flow can balance the local selective pressures which continually decrease the frequency of alleles introduced through gene flow. In most cases this complex set of genetic and environmental interactions can only be maintained temporarily, making this form of balanced polymorphism transitory or impermanent.

Balancing selection also helps to maintain genetic polymorphisms. In the case of balancing selection, though, the maintenance of polymorphic frequencies is the result of the increased fitness of heterozygous individuals—a process known as heterosis or hybrid vigor. Homozygotes (for either allele) have a lower fitness than the heterozygotes in the population. This form of selection is inherently more stable, since it is based on the interaction of fewer, more localized, and more specific environmental and genetic factors.

Given the plesiomorphic nature of the ABO blood group system and its polymorphic persistence in the gene pools of the various members of the Hominoidea, it is reasonable to assume that some form of diversifying or balancing selection might be responsible. Unlike well-documented examples such as the maintenance of sickle cell anemia in populations exposed to falciparum malaria, however, there are no known environmental factors which could account for differential phenotypic fitness in the ABO system due to heterosis. The present interpretation, of course, does not rule out the possibility that some form of heterosis may nonetheless be responsible for the maintenance of polymorphisms in the ABO system. However, it must be remembered that in order to accept such a hypothesis one needs to present and document more than a simple and unsupported suggestion.

Similar objections can be advanced for supposing that ABO polymorphisms are the result of diversifying selection, although as I have indicated, the widespread maintenance of polymorphic frequencies in several lineages presents strong suggestive evidence for the operation of either diversifying or balancing selection at this locus. Additional and more specific suggestions will be made in this regard below, when I discuss some of the correlations which have been proposed between specific alleles of the ABO system and the incidence of various diseases.

Among the four types of selective processes listed, directional selection seems to be the most appropriate selective process which could account for the contrasting gene frequencies of type B blood between East Asian and Native South American populations. Directional selection will lead to a shift in gene frequencies when a change in the environment or the migration of a population to a new environment results in a shift in the fitness value of individual alleles. Thus, the impetus for directional selection can be traced directly to some specific environmental factor or combination of factors which originate from contrasting environmental conditions. Kettlewell's (1961) study of industrial melanism in the peppered moth is a classic example of the operation of directional selection.

Turning our attention to the relationships which have been posited in the literature between gene frequencies in the ABO blood group system and various correlated factors, we may begin by noting that no single published study to date has presented unambiguous evidence which might demonstrate the operation of directional selection acting on this gene system. This situation is not surprising, especially in light of the fact that in the vast majority of modern human populations and among our closest primate relatives the ABO system continues to persist in polymorphic frequencies. As Weiss and Mann (1990:485) have stated,

> It is doubtful that we will soon find a final answer on selection and ABO. Anthropologists and serologists have written hundreds of papers on this topic and have designed clever techniques to detect selection, but the problem's

inherent complexities have defied a full answer.

Indeed, the ABO blood group system was the first blood group system to have been discovered and the first to have been studied in relation to environmentally-engendered selective factors. In the ninety years since its discovery numerous studies have attempted to demonstrate correlations between specific diseases and their frequencies among individuals possessing specific phenotypes in the ABO system. Most recent texts discussing modern human genetic variation include summaries of the results of these studies (cf. Bodmer and Cavalli-Sforza 1976; Brues 1977; Buettner-Janusch 1973; Harrison et al., 1988; Levitan 1988; Molnar 1983; Mourant et al., 1976; Nelson and Jurmain 1991; Weiss and Mann 1990), and therefore no attempt will be made here to summarize these results in detail.

Generally speaking, correlations have been demonstrated at varying levels between ABO phenotypes and the incidence of chronic diseases, infectious or epidemic diseases, and the incidence of hemolytic disorders due to maternal-foetal incompatibility. Unfortunately, with the exception of hemolytic disorders, most of the other correlations suggested by individual studies have been challenged when other researchers have failed to replicate their results. In some instances a weak level of significance has been sustained as a result of subsequent studies, while in other cases attempts at replicating the initial reported findings have either failed or have actually contradicted the original results.

A broad category of diseases including various chronic disorders have been associated with individual ABO phenotypic variants. Among some of these are various tumors, cancers, ulcers, diabetes, and heart disease. It has been repeatedly pointed out (cf. Mourant et al., 1976:127; Buettner-Janusch 1973:429; Harrison et al., 1988:273; Nelson and Jurmain 1991:136) that these types of diseases are typically associated with the post-reproductive period and are therefore not likely to exert significant selective pressures which can result in gene frequency changes. Furthermore, as Bodmer and Cavalli-Sforza (1976:340) note, even if these conditions are viewed as selectively significant agents, their overall effects are very slight.

A second class of diseases for which correlations with particular ABO phenotypes have been claimed includes various infectious or epidemic diseases. Some of these diseases include smallpox, bubonic plague, leprosy, syphilis, tuberculosis, infantile diarrhea, and streptococcus-related rheumatic fever.

Correlations to these diseases would certainly be more significant in as much as the incidence of these diseases could directly influence reproductive and pre-reproductive members in a population. Unfortunately, despite numerous attempts to demonstrate correlations, none of these associations has yet been substantiated (cf. Bodmer and Cavalli-Sforza 1976:340; and Nelson and Jurmain 1991:136). Nevertheless, some researchers continue to cite such studies. Weiss and Mann (1990:484) report that

> One set of data, gathered in India, where smallpox still existed, supports the hypothesized similarity of A antigen

> and a structure on the smallpox virus. In the rural, unvaccinated Indian population, the researchers found a much higher incidence of smallpox among type A and AB people than in O and B individuals. Not only was the disease more common in the A/AB group, but it was also more often severe or fatal in people with the A antigen. Poor medical care raised the mortality rate in the A/AB group to 50 percent.

Such results, if corroborated by other investigators and in other instances, do in fact appear to be formidable. A mortality rate of 50 percent among members of the A/AB group by itself, however, does not express a ratio to the mortality rate of the O or B groups during this epidemic. Mourant et al., (1976:127), who have reviewed these studies, point out that other researchers who have conducted numerous investigations on these proposed relationships between individuals with the A antigen and the smallpox virus have failed to substantiate these correlations. Furthermore, one may note that the gene frequencies of the allele for type A blood in India range between 15 and 25 percent—a set of values which straddles the world average values for this allele. Given the proposed selective disadvantage of individuals with type A blood in the presence of smallpox, these gene frequency values are particularly difficult to defend.

Finally, there is one more example of interest in relation to the suggested relationship between increased mortality of type A and AB individuals and the incidence of smallpox. In North America, the highest gene frequency values for the allele for type A blood occurs among the Blackfoot Indians. As seen in Appendix 1, Table 21, gene frequency values in excess of 80 percent have been recorded for the Blackfoot and Blood Indians. Gene frequency maps for this allele clearly show clinal distributions of rapidly decreasing frequency for this allele surrounding these groups.

This information is presented here in light of the historically-recorded decimation of the Blackfoot Indians by smallpox nearly two hundred years ago as recorded by David Thompson (1962:236-237, and 245-246), in which at least sixty percent of this group perished. Such massive casualties due to smallpox among Native American groups during historic times are not excessive when compared to similar episodes among other Native American groups. What _is_ significant in this context, however, is that if the smallpox virus _had_ in fact differentially decimated a disproportionately larger percentage of the individuals with type A blood, then we would hardly expect this group to show the enormously high gene frequency for the A allele in their descendants which has been documented by modern serological studies. It is precisely these types of incongruities in the data which continue to plague most attempts at correlating the incidence of infectious and epidemic diseases with particular phenotypic variants in the ABO blood group system.

Despite the mass of contradictory and inconclusive results which have characterized past attempts to establish relationships between diseases and various phenotypic forms in the ABO system, indirect evidence seems to point to the existence of some type or types of selection affecting various alleles of the system. Although the precise nature and mechanisms involved remain inconclusive, the evidence derived from the study of the incidence of hemolytic disorders is highly suggestive.

Hemolytic disorders resulting in foetal death and miscarriages early during pregnancy have been documented for mother-foetus incompatibilities both in the Rh and the ABO blood systems. Foetal incompatibility in the ABO blood group system alone (Mourant et al., 1976:126), or in the Rh system in conjunction with ABO blood system compatibility (Bodmer and Cavalli-Sforza 1976:338-339), have been well documented.

In each of these cases it is clear that the continued incidence of such cases of incompatibility will eventually lead to the elimination of the A and B alleles from the gene pool. This realization prompted Mourant et al., (1976:127) to suggest that despite the contradictory and inconclusive data available on selective advantages associated with the alleles for A and B blood types, some (as yet undetermined) mechanisms must be operating in such ways so as to confer some type of selective advantages to these alleles. This argument was necessary to account for the continued maintenance of the alleles for A and B blood types despite the selective advantages of the allele for type O blood which result from foetal hemolytic disorders. Were this not the case, the alleles for A and B blood types would have been eliminated from modern populations.

Despite the apparent strength of this argument, an alternative interpretation must also be considered. Brues (1963 and 1977) has argued that beyond the documented advantage of the allele for type O blood conferred by hemolytic disorders, the potential selective advantages or disadvantages conferred by selective forces acting on specific alleles A and B do not constitute the requisite mechanisms necessary for the maintenance of a balanced polymorphism. According to Brues (1963:288)

> This is not a description of a balanced polymorphism, but a fortuitous polymorphism which would possess no positive stability under natural conditions. A stable polymorphism requires that there be some positive force tending to restore equilibrium when deviations have occurred...

In a series of computer-based mathematical models, Brues (1963) demonstrated that the observed distribution and gene frequencies of the alleles of the ABO system in modern human populations worldwide could only be the result of some form of heterozygote advantage of the genotypes AO, and BO, rather than the result of selective advantages associated directly with the alleles A and B alone. Based on these results Brues (1963:298) states,

> It is concluded that a selection pattern consistently favoring heterozygotes over homozygotes, with a maximum selection differential of approximately 100/66 (AB vs. BB) was necessary to maintain the known distribution of ABO gene frequencies, through the small-population stage of human history. As a corollary it is noted that regional or secular varia-

> tions in the selective value of various genotypes must have been relatively minor, since alterations sufficient to reverse the heterozygote-over-homozygote advantage result in rapid decay of the polymorphism. It is further pointed out that in view of the fact that maintenance of a polymorphism is basically dependent on a difference in selective advantage between homozygote and heterozygote, the collection of clinical data in which phenotype only is available, is of dubious value; and that more intensive and less extensive studies, in which family data made some distinction of genotype possible, might be more productive.

Although this study by Brues is now almost thirty years old, the predictions of her study appear to be strikingly prophetic in describing the fate of studies focused on correlations between particular phenotypes of the ABO system and various diseases. Indeed, in retrospect, it seems that the long-standing balance between the various alleles of the ABO blood group system in the apes and in human populations—despite the selective forces due to the effects of hemolytic disorders—can only be accounted for through some form of balanced polymorphism which, as Brues points out, can only be the result of heterozygote advantage.

Beyond these observations we may also note that if the calculations and conclusions presented by Brues (1963) are equal to the apparent accuracy of her assessment of the selective mechanisms involved in maintaining a polymorphic distribution of gene frequencies for the ABO system, then the selective advantages associated with the heterozygous genotypes will not readily be traceable to specific and environmentally contrastive variations which—as Brues points out—would have quickly eliminated one or more alleles in different environments, negating the widespread polymorphic distribution observed in modern populations.

This is not to say that there are no external selective components operating in a set of processes which result in the maintenance of a balanced polymorphism with varying gene frequency values in different environments. In fact, external selection directed at genotypic classes resulting in balanced polymorphism appears to be the most reasonable interpretation of the combined weight of distributional, experimental, and clinical results in the literature. Rather, it seems that the environmental components responsible for maintaining the observed gene frequency levels tend to be fairly constant and non-contrastive in their effects in different environments.

Additionally, we may note that such effects become particularly evident in larger populations or so long as interbreeding or gene flow unifies discrete local populations into regional networks within which selective pressures can express themselves. Once isolated, smaller populations—though still subject to selective forces—will likely experience unique, stochastic variations in gene frequencies which permit the loss of one or another allele in the system. Mourant et al. (1976:127) note that populations isolated on islands and mountainous areas tend to lose one or more of the alleles in this system. These changes occur among relatively small and genetically isolated populations. Such shifts in gene frequency are best interpreted as the result of genetic drift or sampling error.

Thus, in evaluating the evidence for the action of selective mechanisms acting on the ABO blood group system as a whole, the collective weight of these observations leads me to conclude that we must reject the effects of directional selection in accounting for the observed contrasts in the gene frequencies of the allele for type B blood in East Asian and Native South and Central American populations. Of the original set of hypotheses proposed to account for these differences, only the possibility of sampling error remains to be addressed. The effects of balancing selection, however convincing, will tend to maintain polymorphisms, negating the observed contrasts in gene frequencies. Clearly, some other mechanism must be responsible for the observed differences in gene frequencies between these geographical areas.

In the preceding discussions I proposed a set of multiple hypotheses which might account for the observed differences in gene frequency values in the ABO blood group between East Asia and the Americas. Subsequent discussions served to eliminate all of the stated hypotheses except one. This last hypothesis states that the observed differences in the ABO blood group data are the result of sampling error. The following section will consider this alternative, and provide mathematical projections based on this hypothesis which can be used to yield an estimate of the size of the initial entry population.

4.3.8 THE INITIAL ENTRY POPULATION: ESTIMATE OF SIZE

The mechanism of sampling error, unlike the previously described alternatives, is not dependent upon extrinsic factors subject to individual testing and documentation. Sampling error has been consistently documented in a wide variety of gene systems and populations. The action of sampling error is essentially a universal and random process, limited only by the requirement that a gene system should have more than one allele with frequencies greater than .000. The effects of sampling error are further increased as the size of the population is decreased. In effect, we can not exclude the operation of sampling error—and thus, we must pursue this hypothesis in explaining the observed differences between the gene frequencies of East Asian and Native Central and South American populations.

The term "sampling error" is used here preferentially over more specific terms such as genetic drift or founder effect. In this context it is significant to note that the inherent genetic mechanism in all these processes is the same (cf. Ayala and Valentine 1979:118; Bodmer and Cavalli-Sforza 1976:391-392; and Levitan 1988:270). This preference is justified by the argument that my intended emphasis in the subsequent discussions will be on the inherent mechanism which accounts for random shifts in gene frequencies, rather than on additional conditions or constraints which (in the context of biohistorical discussion) may or may not be evident or relevant.

As I have tried to indicate earlier, the average gene frequency of the allele for type B blood in East Asia is about twenty percent (gene frequency of .2). Typical values range from lows of about .15 to highs of nearly .3 in central Asia; while a survey of the values for allele B in East Asia as reported in Roychoudhury and Nei (1988) shows a range from .15 to .25 with a modal value of .21 and a mean of .20 for native populations. The present discussion assumes that this value (.2) represents an average stable polymorphic value in a large population for this environment. I am further assuming on this basis that this value represents a realistic value for the gene frequency of this allele in East Asia in the past, although actual corroboration of exact values for past populations is not directly possible.

Assuming, then, that the source population for the native peoples of the New World was characterized by a gene frequency of approximately .2 for allele B ($p = .2$) and a combined frequency for alleles allele A1, allele A2, and allele O, ($q = .8$), we can calculate the probability that such a population could produce a descendant population with values of $p = 0.0$ (or $< .01$) and $q = 1$ (or $> .99$) through sampling error by using the formula:

$$s = \frac{pq}{2N}$$

(see Levitan 1988:268; Bennett 1979:128-29). If the value yielded through this formula is tripled, we include all possibilities within three standard deviations from the mean, or over 99% of the possibilities.

Sampling error, however, could either decrease or increase the frequency of alleles with equal chance. In other words, the probability of either increasing or decreasing gene frequencies in a situation of sampling error is .5 in each case. Furthermore, the process of sampling error is highly sensitive to population size (the value of N), so that large populations show a negligible impact, whereas small populations can show major variations in gene frequencies.

Whatever we may conceive the size of the parental East Asian population to have been from which the ancestors of the first Americans were derived, it is likely that some fragmentation and ultimately isolation of populations were likely to have occurred prior to entry into the Americas as suggested by Wolpoff et al., (1984). The formula presented above provides a means by which we may estimate the size of the smallest population isolate (in a sequence of populations or generations) which ultimately gave rise to the ancestors of the modern Central and South American Native populations.

Given the dynamics associated with the "center and edge" model (Wolpoff et al., 1984:450-455), we would not expect the parental population to be very large. However, for demonstration purposes we shall begin with an arbitrarily large number.

Using the current area of the State of Alaska to represent the area originally occupied in Beringia by the source population of New World peoples, with a hypothetical population density of 0.005 inhabitants per square kilometer (see population density figures used by Wobst 1976:50 for Paleolithic hunters elsewhere in the Old World), we obtain an (admittedly arbitrary) source population figure of 7,653 individuals (N = 7,653).

Returning to our formula, we can now substitute these figures as follows:

$$s = \frac{(.2)(.8)}{2(7,653)} = 0.00323$$

Multiplying this figure by 3 to achieve the 0.01 level of confidence, we get 0.00969. This can be interpreted to mean that given a population (N = 7,653) with gene frequencies for (allele B) p = .2 and (alleles A plus O) q = .8, sampling error will likely produce a descendant population (F1) with p values between 0.19031 and 0.20969. This is a statistically small variation due to sampling error.

Assuming that sampling error produced a decrease of 0.00969 in p for this hypothetical population, and assuming that the population numbers (the value of N) remains constant, and assuming that genetic drift continues to decrease the value of p in each subsequent generation without reversal, we may repeat our calculations using the above formula, calculating our figures to the 0.01 level of confidence each time. This procedure yields a value of p for the F2 generation of 0.18079; a value of 0.17146 for the F3 generation; 0.16232 for the F4 generation; 0.15338 for the F5 generation; and so on, until in the F30 generation we finally reach a figure < 0.01 (the actual figure is p = 0.00824.

Since sampling error requires thirty generations to reduce the value of p from the original .2 to < 0.01; and since at each generation the random process of sampling error could (with equal probability) either increase or decrease gene frequencies of an allele (although we have assumed a constant decrease, generation after generation), we may express the probability that such a source population could produce a descendant population with a p value of 0.0 or < 0.01 as the product of the chances for a decreased value of p in each generation or (.5) raised to the 30th power. Therefore, given a source population with a p value of .2, and N = 7,653, the chances that it will produce a descendant population with a p value of 0.00 or < 0.01 is 1/1.0737 X 10 to the 9th power or 9.3132 X 10 to the negative tenth power—an enormously unlikely set of events.

Utilizing this same procedure and the same assumptions, but changing the value of N (the parental population size) in our computations, we can calculate alternative outcomes. Table 4.2 shows some typical results.

Regardless as to the route of entry (the coastal route, or through the ice-free corridor), the geographic, climatic and environmental restrictions imposed by either route of entry would have produced what is known as a "bottleneck effect," severely restricting the number of individuals who could proceed south of the continental glaciers, generation after generation. Such a restriction in population numbers without the actual migration of the source population implicit in the related concept of "founder effect," would have resulted in extensive possibilities for genetic drift, given a restricted population size. This is precisely the process predicted by the figures in Table 4.2.

Once the descendants of this population progressed to environmentally more favorable or productive areas, their numbers would have increased, perhaps slowly at first in response to slightly improved conditions. Later generations of descendants would have continued to build upon this numerical trend.

The figures in Table 4.2 and the techniques used in compiling these figures allow us to estimate the magnitude of the population size restrictions imposed. These figures in turn will allow us to estimate the relative size of the immediately subsequent descendant population—a figure which is not likely to have been significantly different.

Table 4.2

Initial Entry Population Size Estimates

N =	Number of Generations	Probability*
7,653	30	9.3132×10^{-10}
800	7	0.0078125
750	6	0.015625
475	5	0.03125
100	2	0.25

* This column gives the probability value that an East Asian population of a given size (N) with a gene frequency for allele B (p value) of .2 could have given rise (through sampling error) to a descendant population where p = 0.00 or < 0.01. For example, given a parental N value of 100, two generations, and a 1/2 chance in each generation, we can calculate 1/2 X 1/2 = 1/4 or 0.25—a 25% chance that a parental population of this size (with p = .2) could have given rise to a descendant population (with a gene frequency value of p = 0.00 or < 0.01) at the .01 level of confidence. See text for additional discussion.

Based on these figures (calculated to the 0.01 level of confidence), there is less than 1 chance in 100 that the population immediately preceding the entry population numbered 800 individuals or more. Modeling human geographical distributions and mating systems for band level hunters and gatherers, Wobst (1976) proposes an optimum figure of 475 individuals. As Table 4.2 shows, there is only slightly over 3% chance that the source population for the New World was this large. Smaller figures are statistically more likely to yield the observed frequencies in living populations. However, in the absence of clearly established minimum numbers for a viable human population, I am reticent to use a much smaller number. Furthermore, genetic studies of founder populations in historic times with small numbers of individuals seem to indicate a rapidly escalating scale of problems associated with homozygosity (Mayr 1970:305; Dobzhansky, Ayala, Stebbins and Valentine 1977:157-160; and Molnar 1983:137-139) which has not been documented for modern native Americans.

Certainly, given sufficient time, and assuming that the associated problems did not lead to the extinction of the descendant populations, the problems of homozygosity could have been resolved. Perhaps they were. However, this lies outside the realm of demonstration, and must therefore remain speculative.

4.3.9 ESTABLISHING A MINIMUM TIME GRADIENT

Building upon the foundations which have been established in the preceding section, and employing the demographic formulas introduced in the previous chapter (section 3.4 and subsequent discussions), we may project some tentative values bearing upon the rates of population growth of early New World populations and the estimated time depth of initial entry into the Americas. These approaches can be made to yield a reasonable estimate of initial entry, while at the same time allowing us to evaluate the validity of alternative chronological projections.

Without repeating in detail some of the discussions presented in the preceding chapter, it would be helpful to recall some of the caveats which pertain to the use of demographic projections. Of these perhaps the most fundamental observation relates to the difference between demographic growth rates in small or even regional populations over short time spans, as opposed to average demographic growth rates based on continental or larger areas over thousands or even tens of thousands of years. Most demographic work (cf. Bogue 1969, Weller and Bouvier 1981) is based on grouped data and statistical sampling which forms the basis for mathematical projections. This consideration is equally true for calculations of growth rates as it is for estimates of population size (even modern population numbers are derived this way since there is no comprehensive census data for the world as a whole).

Discussions including world population numbers and growth rate values (r values) have been presented by numerous authors including Coale (1974); Ehrlich and Ehrlich (1970); Bogue (1969); Weller and Bouvier (1981); Clark (1977); and Weiss (1984). Additional discussions which include estimates of population densities in modern ethnographic and prehistoric hunter-gatherer societies (together with discussions of mating networks) have been provided by Wobst (1976). On the basis of these data the following preliminary considerations of world population density and growth values in prehistory can be posited.

In order to establish a realistic base line for demographic growth rates in prehistory, it is necessary to begin with a realistic set of estimates of the geographic distribution and density of human populations over a wide area during an extended period of time. For this purpose I have adopted the following working assumptions:

1) By 1,000,000 years ago, bands of hunter-gatherers of Homo erectus had spread through parts of Africa, Asia, and Europe. It can be assumed that not all areas would have been inhabited that early. Therefore as a rough estimate, I have assumed that 60% of Africa and 30% of the Eurasian continental land masses contained human populations. This represents a total area of about 21,378,000 square kilometers.

2) Wobst (1976) has argued for a minimum hunter-gatherer population density value of 0.005 persons per square kilometer, with most observed values for ethnographic examples exceeding this figure by up to a factor of ten (0.05 persons per square kilometer). Multiplying the low value by the total area (21,378,000 square kilometers) we obtain a total human population value of 106,890 persons—a value which incidentally is below the 125,000 figure given by Ehrlich and Ehrlich (1970:9) for australopithecines in Africa alone.

3) Finally, based on a wide variety of sources discussed in the previous chapter, the world population value of 8,000,000 for 8,000 B.C. (about one million years later, just before the rise of agriculture) will be used as the terminal figure in this sequence. From these values, world population growth rates for hunter-gatherers can be estimated.

Throughout these calculations I have attempted to be both realistic and conservative (see section 3.2, pages 93-105 of this work for world population figures and definitions of the symbols used below). Since setting the population values at time zero (No at 1,000,000 years ago) at a low figure is the most effective way to obtain higher growth rate values, and since these growth rate values will be used to project growth rate values for the New World, I have also reduced the original population value of No. Instead of using a value of 106,890 persons, I have used a value of 50,000. This figure is equivalent to decreasing population density values below 0.0025, or decreasing the geographical base to less than half of the original estimate given above.

Setting the No value at 50,000 (see above); the Nt value at 8,000,000; and the time interval (t) at 1,000,000 years we obtain a growth rate value (r) of 0.0000051. This figure translates to a doubling rate of one doubling every 136,576 years. This value forms the base line figure for average growth rates for hunter-gatherers in prehistory which I will use hereafter.

However, not all of prehistory saw such slow growth rates. In the period between 10,000 and 2,000 years ago, the domestication of plants and animals and the rise of settled life and early civilizations drastically changed the growth rates of human populations in the world. During this interval of time, world populations rose from 8,000,000 to 300,000,000 (see Coale 1974, and earlier discussions in chapter 3). Using these values we obtain a growth rate (r) of 0.000453, and a doubling time (t) of 1530 years. This second set of figures constitute our base line values for population growth rates after the beginnings of settled agricultural life.

In order to apply these figures to calculate time depth of initial human occupation in the Americas we need several other values, and we must incorporate several other considerations. One of the fundamental values we must use is the total human population in the Western Hemisphere at circa 1500 A.D. (1492 A.D.). Summaries of estimates by various authors can be found in Denevan (1976) and Stewart (1973).

Realizing that the length of time of human occupation of the Americas will be a function of the estimated size of human population in 1500 A.D., I have once again attempted to use a conservative value. After reviewing various population values in the literature, Stewart (1973:35) concludes that "obviously, all that can be said at present about the approximate size of the native American population of 1492 is that it was between ten and a hundred million, which is indeed a very broad approximation." I have chosen to use the low end value in this range—that is—10,000,000 people circa 1500 A.D.

Still other factors need to be stated. Among these are the approximate date when plant cultivation began in the Americas, and the size of the initial population which entered the Americas. The first of these can be approximated at 6,000 B.C. Fagan (1986:286) estimates the beginnings of this period at 5,500 B.C., and Wenke (1990:257) gives a date of 5,700 B.C. The size of the initial entry population is seldom given a numerical value, though most authors feel that it was relatively small. On the basis of the calculations relating to sampling error in the ABO blood group (earlier in this chapter) I have calculated an initial entry population size of about 100 people. Interestingly, Mosimann and Martin (1975:305) used this same figure—although without offering any explicit justification for their choice.

Finally, as a general statement, and without discussing in detail the responsible mechanisms, we may project that the rate of population growth following the initial entry event must have been somewhat higher than one would expect for subsequent periods when population equilibria had been established. More will be said about this matter in the appropriate sections which follow.

Beginning, then, with an No value of 10,000,000 in 1500 A.D., and assuming that we must use the growth rate value (r) of 0.000453 for settled agriculturalist periods, we may work backwards from this figure as follows. The period of time between 1500 A.D. and 6,000 B.C. (Period III in this sequence) represents 7,500 years. Multiplying this figure by the growth rate of 0.000453 we get 3.3975. The natural antilogarithm of 3.3975 is 29.889283. Dividing 10,000,000 by 29.889283 gives us the estimate of 334,568 people at minimum in the New World in 6,000 B.C. This figure would translate to an overall population density for the whole of the New World of about 0.0126 persons per square kilometer at the onset of Period III, 8,000 years ago.

Relative hunter-gatherer population density equilibria, however, were probably established at a lower value. Assuming that this value can be set somewhere around 0.005 persons per square kilometer, calculation would yield a total New World population value of about 132,686 individuals. Since this represents a low value based on the numbers presented by Wobst (1976), we may assume that greater-than-normal rates of population growth prevailed during Period II—the interval when total populations values for the New World rose from 132,686 to 334,568 persons. In order to account for this increased growth rate, I have used a growth rate value (r) which is ten times the calculated base line value for hunter-gatherers worldwide (i.e., 0.000051).

Dividing the population size of 334,568 people in 6,000 B.C. (the Nt value) by 132,686 people—at the time that populations reached a density value of 0.005 persons per square kilometer—we get 2.5215019. The natural logarithm of this number is 0.9248547. If we now assume a tenfold increase in the growth rate of New World hunter-gatherers for the intervening period, we must divide this last number by 0.000051. This calculation gives us a time interval of 18,134 years for the length of Period II.

Finally, it is reasonable to suppose that the absence or decreased competition by neighboring bands might have further accelerated the potential for population growth during the period following the initial entry of people into the Americas (Period I). How much these conditions accelerated population growth rates is uncertain; however, several factors might be mentioned. Whereas decreased pressure for space and resources might have increased growth potential, the dynamics of hunter-gatherer socio-economic organization described in the previous chapter which low population densities helped to maintain, would have militated against the sorts of explosive growth patterns represented by some of the mathematical model outputs presented by Mosimann and Martin (1975) (also see discussions in Chapter 3).

There is no established procedure which might be used to yield defensible arguments for the scale of growth rates which might be employed. Therefore, as a rough approximation of the effects of low resource competition curbed by the socio-economic limitations of band level hunter-gatherer adaptations we might hypothetically increase the world hunter-gatherer growth rate value of 0.0000051 by a factor of one hundred. Thus, the growth rate (r) which I have chosen to project for this period will be set at a value of 0.00051.

The population growth rate value of 0.00051 is only a rough approximation. Whereas actual growth rates may have been lower or higher, it is much more likely that they were lower (more typical of hunter-gatherer population growth rates), although actual values were probably considerably above equilibrium values. The arbitrarily chosen growth rate of 0.00051 used for Period I exceeds the calculated world growth rate (0.000453) for early settled agriculturalists, though it does not support the types of explosive growth suggested by other authors (e.g., Mosimann and Martin 1975).

Using a population figure of (Nt) 132,686, and an initial population value (No) of 100 persons, and a growth rate value (r) of 0.00051, we can calculate that this initial period spanned an interval of 14,099 years. On the basis of all these calculations we can therefore project that the initial entry of people into the New World took place minimally about 40,000 years ago (see Table 4.3). Using an alternate growth rate (r) value of 0.0003, we may project that the initial entry of peoples into the Americas took place around 50,000 years ago.

By 26,000 years ago, populations would have begun to reach hunter-gatherer equilibrium densities averaging about 0.005 persons per square kilometer. During the next 18,000 years, population growth rates would have stabilized—though still at significantly higher rates then worldwide base line rates for hunter-gatherers. Finally, with the advent of plant domestication and settled existence beginning around 8,000 years ago, population growth rates would have increased once again until European contact in 1500 A.D.

Table 4.3

New World Time and Demographic Growth Rate Projections

Period	Beginning	Ending	Growth Rate	Population Size
I	40,000+ B.P.		0.00051*	100
		26,000+ B.P.		132,686
II	26,000+ B.P.		0.000051	132,686
		8,000 B.P.		334,568
III	6,000 B.C.		0.000453	334,686
		1500 A.D.		10,000,000

* As noted in the text, this is probably an excessively rapid growth rate (r) value. If the growth rate during this first period was (r = 0.0003) sixty times the calculated worldwide average growth rate of hunter-gatherers (or about two-thirds the calculated growth rate characteristic of early settled agriculturalist times), then the beginning of Period I would have to be set at around 50,000 B.P.

Using a population figure of (Nt) 132,686, and an initial population value (No) of 100 persons, and a growth rate value (r) of 0.00051, we can calculate that this initial period spanned an interval of 14,099 years. On the basis of all these calculations we can therefore project that the initial entry of people into the New World took place minimally about 40,000 years ago (see Table 4.3). Using an alternate growth rate (r) value of 0.0003, we may project that the initial entry of peoples into the Americas took place around 50,000 years ago.

By 26,000 years ago, populations would have begun to reach hunter-gatherer equilibrium densities averaging about 0.005 persons per square kilometer. During the next 18,000 years, population growth rates would have stabilized—though still at significantly higher rates than worldwide base line rates for hunter-gatherers. Finally, with the advent of plant domestication and settled existence beginning around 8,000 years ago, population growth rates would have increased once again until European contact in 1500 A.D.

I feel that this set of projections is both realistic and conservative. It is quite likely that the time depth estimated in this demographic model output represents a set of minimum values for the timing of the peopling of the Americas, with the actual date of initial entry being significantly older. However, for the argument, let us briefly consider the opposite alternative.

If we accept a scenario in which the initial entry event took place in 9,500 B.C. (11,500 years ago), we may consider the consequences of such an assumption. Beginning with a population size of 334,568 people in 6,000 B.C. we would find that the 3,500 year interval between initial entry by 100 people and the onset of settled life would require the operation of an average growth rate (r) value for this period of 0.0023187. This value is over five times the projected world growth rate for early agriculturalists up to Ancient Roman times; and it is roughly equivalent to the world population growth rates between 1650 A.D. and 1850 A.D., using the same conservative rate estimates used earlier. Clearly such an assumption needs to be reconciled with demographic facts if it is to be accepted.

The significance of these calculations points to the likelihood that the current, archaeologically based date of 11,500 B.P. for the initial entry, as represented by Clovis hunters, is most likely incorrect. The estimated growth rate of 0.00051 applied to Period I is no more than a realistic hypothesis. Figure 4.1 (below) shows the growth curve derived from the figures presented in Table 4.3. The growth curve shown in Figure 4.1 for the whole of the Americas is not significantly different from the world population growth curves spanning the last 1,000,000 presented by Coale (1974:42).

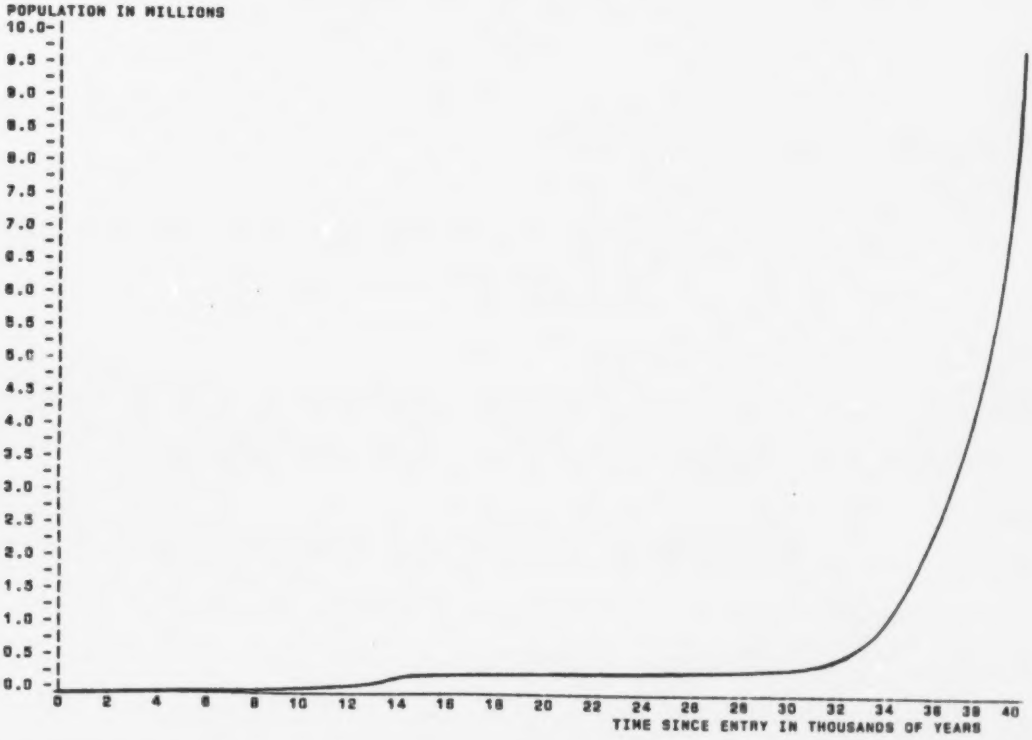

Figure 4.1
Hypothetical Population Growth Curve
The growth curve numbers are based on the values given in Table 4.3.

If we had used a growth rate value of 0.00005 throughout the first two periods, the main difference in the growth curve would have been the lengthening of the horizontal axis yielding a greater time depth. Using an initial entry population figure of 100 people, the initial entry date would fall around 167,000 years ago. If the initial entry population figure was set at 800 people, the initial entry date would fall around 126,000 years ago. Both of these estimates ignore the positive impact exerted on population growth by the lack or decrease in competition for resources by neighboring groups. Both of these estimates are also clearly incompatible with the entry date estimates yielded by some of the other methods discussed earlier in this chapter.

4.3.10 PHYSICAL ANTHROPOLOGY, DEMOGRAPHY, AND CHRONOLOGY: A SUMMARY

On the basis of the preceding discussions in this chapter we may offer the following conclusions regarding the biohistory of the peopling of the Americas. As seen in the discussions of the fossil evidence, and based on the current knowledge of fossil specimens from the Far East and the Americas, we may suggest that the Americas were peopled by groups entering from East Asia, with the earliest time for the initial entry into the New World occurring after about 100,000 years ago, and most likely after 75 thousand years ago.

The calibrated genetic distance values presented by Cavalli-Sforza et al. (1988) (see Table 4.1) suggests an initial entry date range between 37,481 to 48,889 years ago. As was stated earlier, these figures agree fairly well with an entry date of around 38,000 years ago derived through the recalculation of the mitochondrial DNA data presented by Cann et al. (1987), using Wolpoff's (1989a) approach based on Nei's (1985, 1987) base pair substitution rate.

The most reasonable interpretation of the available evidence suggests that sometime between 50,000 and 40,000 years ago a small group of anatomically early modern humans numbering around 100 individuals entered the New World. This conclusion is consistent with our current understanding of paleoanthropology worldwide, and is the result of various independent lines of inquiry (primarily from genetic data) which converge to support this single conclusion. At the same time, this conclusion represents a reasonable conservative estimate which must remain open-ended. No rigid limits are implied by this estimate. At the same time, given the widespread disagreements which exist among American archaeologists based on different interpretations of archaeological data as to the timing of this event, we should note that the lines of inquiry used to reach this conclusion were completely independent of any temporal estimates based on American archaeological data.

Additional evidence from studies of mitochondrial DNA by Torroni et al., (1992) seem to corroborate these temporal conclusions. Also, demographic projections based on population data and growth rates further corroborate an entry date in the 40,000 to 50,000 year range. Although exact estimates of the date of initial entry are more difficult to defend from demographic projection, this approach yields a gradient of likelihood which suggests significant decrease in probability for younger dates approaching the Pleistocene/Holocene boundary period. In this context, the population growth rate adopted for Period I (Table 4.3) represents no more than a likely compromise value which can be used as a starting point for generating very rough demographic projections.

The demographic projections presented in Table 4.3 were calculated conservatively at each step in an overt attempt to reduce the projected length of occupation of the New World, and to incorporate the dynamics of low population to resource ratios within the context of the dynamics of hunter-gatherer socio-economic patterns. On this basis, three periods were projected, including an initial period of expansion and rapid growth, followed by a more stable period (still characterized by significantly high growth rates), ending with a third period of renewed rapid population growth following the rise of food production in the New World.

Although preceding sections discussed evidence that suggests the Americas were populated as a result of three separate entry events, no attempt was made to incorporate the possible impact on the demographic model output of entry of the Na-Dene and Eskimo-Aleut groups. As stated earlier, this work focuses on the *initial* entry and early prehistory of the Americas. If the distinctive and contrastive distribution of alleles of the ABO blood system in East Asia and the Americas is the result of sampling error, then initial populations ranging in size between 100 to 750 individuals would have had a 1 in 4 to a 1 in 64 chance to produce these distinctive differences (see Table 4.2). In order to maintain these contrasts through sampling error, subsequent entry events would have had considerably lower probabilities of occurring. Table 4.4 presents some of these probabilities for additional entry events.

Table 4.4

Probability Values for One and Additional Early Entry Events

N =	Probability @ 1st Entry	Probability @ 2nd Entry	Probability @ 3rd Entry	Probability @ 4th Entry
750	0.015625	0.0002441	3.8147×10^{-6}	5.9605×10^{-8}
475	0.03125	0.0009766	3.0518×10^{-5}	9.5367×10^{-7}
100	0.25	0.0625	0.015625	3.9063×10^{-3}

Probabilities for each subsequent entry are calculated by cumulatively multiplying each additional entry event by the initial entry event probability value (e.g., 0.015625 X 0.015625 = 0.0002441 for the second entry event).

Based on the calculations presented in Table 4.4, it can be seen that as additional entry events are considered as part of the population ancestral to the initial "Amerind" group, the possibility that all these groups would have maintained contrastive gene frequencies through sampling error is quickly and severely reduced. Although these calculations do not constitute a definitive demonstration, the figures shown in Table 4.4 strongly suggest that the population which became ancestral to the initial or "Amerind" group represents a single entry event separated from the subsequent entry of the ancestral Na-Dene and ancestral Eskimo-Aleut groups by a considerable temporal hiatus. The geographic range of their modern descendants, on the other hand, suggests that the two later entry events took place at a time when the descendants of the Amerind population had become well established and could significantly resist displacement by later arrivals.

Initially in this chapter, biogeographic and paleoecological topics were introduced. This was necessary in order to provide a review of some of the fundamental factors impinging upon subsequent biohistorical discussions. No attempt was made, however, to discuss these topics in detail, since discussion of relevant factors had to await the establishment of a reliable time-frame for initial entry. Having accomplished this goal on the basis of the biohistorical data, we may now turn our attention to a more detailed review of the paleoenvironmental data and the data from the Pleistocene archaeology of the Far East.

4.4 THE PALEOENVIRONMENTAL CONTEXT

As part of this section it would be desirable to model various paleoenvironmental parameters. Climate, terrestrial and marine glacial geography, paleoecology, and biogeography all contain potentially significant variables pertinent to the peopling of the Americas. Unfortunately, although we may conceive of such cognitive categories as paleoclimatology, paleoecology, biogeography, and glacial geography as separate topics, such topical segmentation belies the essential unity and interdependence in subject and methods which tie these fields together. Reviewing these topics from the single perspective of human population movements and adaptations only serves further to underscore the unity of these topics.

By contrast, paleoenvironmental information is based on an enormously diversified and fragmented data base. These data describe the staggeringly complex interplay of numerous physical and biotic factors which are both time and space dependent, on scales ranging from the short-term and local to global patterns spanning millions of years.

Much of these data are very specific in time, space, and content. From such specific data, patterns and trends of different time and space amplitude must be distinguished; systematic cause-and-effect relationships must be identified, described, and separated from stochastic variations; and a myriad of interacting variables impinging upon the use of specific types of proxy data need to be clarified. Whereas the bulk of the paleoenvironmental literature aims to solve these complex details, the focus in this section on paleoenvironmental information must lie elsewhere.

At the level of broad synthesis it is pointless to pursue discussions of topics which are narrowly focused in time and space. Furthermore, given the thematic focus of this work, it seems appropriate to select a few of the more fundamental issues relevant to the peopling of the Americas; and to exclude other, less directly relevant topics. The most important topics to be discussed, all of which help to unify and tie together various themes, include climates, glaciation, biological communities, and ecosystems during the late Quaternary. These broad topics will be reviewed selectively as they relate to the early human biogeography of the Americas.

4.4.1 ORBITAL GEOMETRY AND EARTH'S CLIMATE

Until the last ten to fifteen years there was little unity among scholars regarding any single overarching driving mechanism which might account for large-scale climatic, glacial, and biotic patterns encountered in the paleoenvironmental data of the Quaternary. More recently, however, there has been an ever-widening acceptance among paleoclimatologists of the critical role played by variations in the Earth's orbital geometry [for historical summaries and a sampling of the literature showing this trend see Flint 1971; Kutzbach 1976; Mitchell 1976; Berger 1978; Schneider and Thompson 1979; Ruddiman et al., 1980; Imbrie and Imbrie 1980; Heusser et al., 1985; Berger et al., 1985; Hecht 1985; Bradley 1985; Ruddiman and Duplessy 1985; Denton et al., 1986; Martinson et al., 1987; and a variety of articles by different authors in volumes edited by Ruddiman et al., 1987 and Fulton 1989].

The fundamental mechanisms invoked by the "astronomical" or "Milankovitch theory" originate in astronomical observations and Newtonian physics. Three independent periodic (or near periodic) variations influence Earth's orbital geometry. These include variations in _eccentricity_ (the shape of the Earth's orbit); variations in _obliquity_ (changes in the angle between the Earth's rotational axis and the plane of the ecliptic—the orbital plane); and _precession_ or precession of the equinoxes (the "wobble" of the Earth's axis which shifts the temporal relations between perihelion and the Earth's seasons). Discussions of these mechanisms relative to long-term climate are presented by Berger (1978:139-142); Schneider and Thompson (1979); Imbrie and Imbrie (1980:943-944); and Bradley (1985:38-46).

Periodic variations or shifts in the Earth's eccentricity, obliquity, and precession result in a _redistribution_ of solar radiation energy (insolation) by latitude and by season—though these variations do not result in a net annual change in insolation for the Earth as a whole (Bradley 1985:41). Variations in insolation values (measured in Langleys) are influenced by all three aspects of Earth's rotation. As a result, the effects of any one of these aspects at a given latitude and point in time may be balanced, accentuated, or negated by one or both of the other aspects. These variations, in turn, influence long-term latitudinally-specific seasonal insolation patterns. An example of such a pattern of shifting insolation values (both seasonally and over thousands of years) can be seen in Appendix 2.

A contrast can be made between earth-bound systems and variations in orbital geometry which operate independently from these systems. The Earth's systems are, generally speaking, intimately interconnected through a series of feedback relationships. This interconnectedness results in recurrent limitations on our ability to establish absolute cause-and-effect relationships. Furthermore, many aspects of proxy records employed in paleoclimatological research are typically subject to repeated reinterpretations as a result of this web of interrelationships. The difficulties associated with repeated reinterpretations can be exemplified by the complex interplay of cause-and-effect relationships which have been ascribed to the growth and decline of glaciation relative to local and global climates (cf. Flint 1971; Kutzbach 1976; Mitchell 1976; CLIMAP Project Members 1976; Gates 1976; Ruddiman et al., 1980; Berger et al., 1985; Ruddiman and Duplessy 1985; Denton and Hughes 1986).

The discrepancies between Earth-bound paleoclimatic explanations, and predictions derived from variations in insolation values have led to the adoption of a number of contrastive terms and concepts. Earth-bound mechanisms are seen as dynamically interconnected, while variations in insolation are said to result in "orbital forcing" of climate. Complex interactions in the Earth's systems also require additional time for the accumulation of measurable orbital forcing effects. Thus, these effects result in a "lag factor" relative to the timing of variations in insolation values. Systematic correlations of this lag factor to the known chronology of orbital forcing effects, in turn, results in a need to develop a consistent "tuning" of such relationships.

One expression of these complex relationships is exemplified by the data presented in Figure 4.2. Monthly departures in insolation values from 500,000 year mean averages by intervals of ten Langleys per day at 60 degrees north latitude (as presented in Appendix 2), have been totaled to obtain a net annual value relative to the mean annual value (dashed line "A" in Figure 4.2). Oxygen isotope values derived from sea-floor samples (widely used as a global record of continental glacial ice mass—e.g., Bryson 1985:275) are represented in this figure by the continuous line (line "B").

Positive correlations between insolation and oxygen isotope values over the last 75,000 years are evident, although there is a clear lag factor present. Furthermore, the correlation at times appears to be imperfect. A simple linear relationship (even allowing for a lag factor) would preclude discrepancies in correlation such as the one seen in Figure 4.2 between 45,000 and 35,000 years ago. This can be viewed as a non-linear response due to the action of intervening variables between orbital forcing and the oxygen isotope record.

Seasonality of departures from mean insolation values can also be significant. The imperfect direct correlation of monthly mean insolation values and oxygen isotope values can be seen in the data presented in Appendix 2.

Imbrie and Imbrie (1980:952) note that the sensitivity of tuning of some of the mathematical models of Earth-bound systems require understanding of single parameters to levels below one percent of

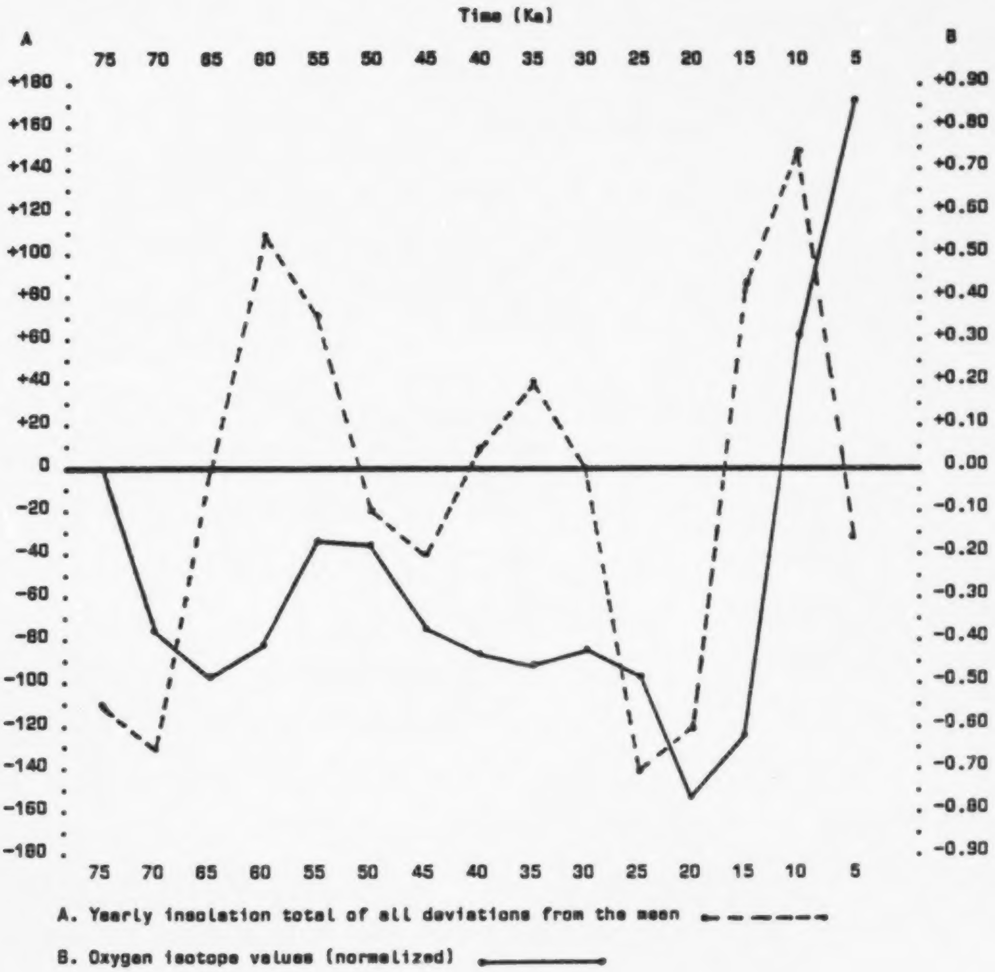

Figure 4.2

Comparative Insolation and Oxygen Isotope Variations.
See discussion in text. Data from Appendix 2, Bradley (1985), Berger (1979), and Martinson et al., (1987).

their known range of variation. In the absence of such fine-grained controls Earth-bound systems may appear to flip or flop unpredictably. Imbrie and Imbrie (1980) ascribe this behavior to sensitivity to initial conditions.

Linearity of response to orbital forcing and decreased time lag values will vary in relation to how directly variations in insolation interact with specific Earth-bound systems. Since oxygen isotope values, climate, glacial mass, and biotic systems are acted upon simultaneously by combinations of critical effects or axes (each having a different proportional significance), the most clearly linear responses to orbital forcing will be exhibited by systems most heavily and immediately effected by variations in insolation rates. The result will be seen in different lag factors in response to orbital forcing. These differences in the nature, degree, and timing of responses to orbital forcing on the Earth's systems, can, in turn, have a major influence on human population movements and adaptations.

4.4.2 ORBITAL FORCING AND LATE QUATERNARY BIOTIC SYSTEMS

One of the fundamental predictions of the Milankovitch theory concerning Earth-bound systems revolves around the seasonal redistribution of annual insolation values. For example, an important component in the growth of glacial ice seems to have been the prevalence of cool summers at higher latitudes during the Pleistocene (Ruddiman et al., 1980:33). Calculations of long-term insolation values over the Pleistocene such as those presented by Berger (1978:145-155) give ample evidence for both short and long term fluctuations in seasonal insolation values. Nevertheless, historic climatic conditions appear to depart significantly from average Pleistocene climates. As Porter (1989:245) points out, it is clear that with the exception of climatic "peaks" (such as the ones presented by the Wisconsinan glacial maximum, and Holocene conditions) "... for most of Quaternary time, environments were intermediate in character." In fact, a growing body of evidence will be discussed indicating that summer and winter seasonal fluctuations would have been less severe, resulting in a more equable climate than now.

When speaking of biotic systems it is useful to recognize that ecologists employ a variety of standard principles and concepts in modeling the behavior of organisms and their relation to their ecosystems. Some of these principles and concepts include Liebig's Law of the Minimum, Shelford's Law of Tolerance, and the Hutchinsonian definition of a niche as a multidimensional hypervolume (e.g., Odum 1971:106-108 and Smith 1986:293). The following discussions and predictions are based on an application of these principles.

Climatic components of habitats, including temperature and precipitation, form integral niche axes which impact individual species. Given a more equable climatic regime during the Pleistocene, it is possible to make certain general predictions:

1) Since each species occupies a multidimensional hypervolume, any single dimension or axis (such as summer/winter temperature extremes) can act as a limiting factor but not as the sole determinant of species distribution. Other ecological axes may be of equal or greater importance in determining potential plant and animal distributions.

2) Pleistocene insolation patterns predict a more equable climate. In so far as this pattern represents a relaxation of stresses due to seasonally extreme temperatures, organismic tolerance of stresses or suboptimal conditions in other niche axes should have been augmented.

3) Since many non-climatic niche axes are not distributed in broad zonal bands, the operation of more equable climates would have tended to increase the frequency of viable microhabitats capable of supporting distinctive localized plant and animal communities.

4) Under modern conditions, climatic zonation by latitude and

altitude interact with niche axes and tolerances of specific species to climatic variables. More equable climates would have resulted in the relaxation of sharp latitudinal and altitudinal boundaries resulting in broad sympatric distributions for species which today show allopatric distributions.

5) Even without an increase in total biomass, increased range size of various species resulting in more numerous sympatric distributions would have resulted in richer, more diversified, ecosystems equivalent to a more fine-grained environment.

It is critical at this point to underscore that these general predictions are possible using standard principles of ecology when the unit upon which predictions are based is the individual organism or species, and its niche relationships. Communities and ecosystems are projected only once organismic responses have been predicted.

By contrast, it seems that many previous paleoecological reconstructions were based upon larger units such as whole communities and biogeographical zones. As a result, difficulties emerged when paleoecological data could not easily be reconciled with paleoecological predictions and expectations. The degree to which the five numbered predictions (above) are borne out by modern reconstructions of Pleistocene paleoenvironments can be gauged by the following brief review of the published literature.

A number of authors have noted that many Pleistocene environments and biotic communities have no modern analogues (Schwert and Morgan 1980:108; Gilbert and Martin 1984:142; Guthrie 1984:260; Graham and Mead 1987:396; and Wright 1987:481). As Graham (1979:52-53) states,

> The biota of the late Pleistocene is characterized by the coexistence of species that are allopatric and presumably ecologically incompatible today. These disharmonious Pleistocene faunas are not restricted to the United States but occur throughout the world (Lundelius 1976). Also, these associations and the climates that supported them may have no modern analogs (Lundelius 1974). Pleistocene disharmonious faunas probably existed during times when the climate was equable and seasonal extremes in temperature and moisture were reduced (Graham 1976).
>
> Even though the late Pleistocene climates were more equable than today, they still supported environmental gradients as indicated by a transect of selected cave faunas from the northeastern to the southwestern United States (Graham 1976). Late Pleistocene environmental gradients, like modern ones, created biotic associations that can be grouped into provinces. However, because of the disharmonious biota of the late Pleistocene, the species composition of these provinces is quite different from modern ones (Guilday et al. 1977) [Original references included].

It is also amply clear that the composition of these distinctive Pleistocene communities and provinces was not the result of some simple mechanism such as generally decreased annual temperatures, or proximity to glacial masses. Schwert and Morgan (1980:108) note that insect populations during the terminal Wisconsinan in the northeastern United States, even within 50 km of the margin of the ice sheets, point to a relatively mild climate. Similarly, in a study of vegetation types in the Mississippi valley, Delcourt et al. (1980:125-128) discuss the persistence of deciduous trees such as oak, beech, hickory, ironwood, ash, etc., together with spruce and pine between 22,000 and 12,000 years ago, at sites ranging from Memphis, Tennessee, to Fayette County, Illinois (60 km from the ice sheet). These examples underscore the unexpected complexity of the data, especially if one were to attempt to explain all of these cases by means of a single or a simple set of mechanisms.

Whereas proximity to ice sheets might initially suggest year-round decreased temperatures, Wright (1987:486) offers the following additional alternative.

> For winters, one might expect the mountain of ice to deliver even colder air masses to the Middle West than does the more distant Arctic today. But a case can be made that the mountain of ice actually blocked the arctic air from sweeping across the plains, and that the air that did descend from the heights of the ice sheet was warmed adiabatically and arrived on the plains dry and windy (Bryson and Wendland, 1967), like the chinook winds that occasionally descend from the Rocky Mountains to the High Plains today... The elimination of winter blizzards with extreme cold might permit the survival of plants and animals sensitive to these conditions. For example, the full-glacial and late-glacial spruce forest of the lower Middle West apparently contained a significant component of north-temperate hardwoods like ash and oak, which are only sparingly represented at the very southern edge of the present boreal spruce forest. White and black spruce are limited on the south by high summer temperatures, whereas the hardwoods mentioned literally freeze and die under conditions of very low winter temperatures. The overlap in ranges for these taxa implies a greater climatic equability than today [Original reference included].

The suggestion of adiabatic warming of air near the ice sheets is further supported by Graham and Mead (1987:376) on the basis of herpetofaunal distributions.

Incidentally, Delcourt et al. (1980:125) give the lower temperature tolerance limit of oak, ash, hickory, beech, elm, black walnut, sugar maple, and other such mesic hardwoods at between -41 to -47 degrees Celsius. Following Wright's arguments, these data point to climatic conditions in which these minimum values would not have been exceeded.

Clearly, older and more simplistic explanations needed revision. Guthrie (1984:260) states that

> The peculiar mixtures of fauna and flora in Pleistocene deposits were once explained simply as a periglacial phenom-

> enon: local ice conditions creating a uniquely cool environment far south where biota from several biomes could mix. It has become apparent, however, that peculiar mixtures of floral and faunal elements occurred not only in central European and central North American periglacial regions, but also in far northern glacial refugia and thousands of kilometers south of the glacial margins (e.g. Davis 1976). Most of today's familiar biotas are without Pleistocene analogue [Original reference included].

Discussing the late Wisconsinan vegetational history of the southwestern deserts of North America, Van Devender et al. (1987:346) add,

> Equable climates would release the lower and southerly range limits of woodland and forest plants and animals but not necessarily extirpate the desert scrub residents, resulting in occasional ecological mixtures that would not be found or would be very unusual today. The hypothesis presented by various authors for lower mean annual temperatures with temperatures equally cooled throughout the year during the Late Wisconsin are not supported.

A similar point is made by Barnosky et al. (1987:312) in discussing the vegetation of the northwestern United States during full- and late-glacial times, when they state that

> ...more precise characterization of past communities, provided in large part by macrofossil data, suggests that zonal displacement was the exception rather than the rule. Compared with present day communities, the glacial vegetation at many sites featured mixtures of lowland and subalpine species that are not found or are very rare on the modern landscape.

Just as we should not expect that either whole Pleistocene climates or biotic communities should have modern analogues, the overall pattern of specific differences which distinguish Pleistocene biotic communities from modern communities is also equally clear. As Martin et al. (1985:25-26) have summarized,

> ...virtually all reported Pleistocene vegetations contain associations of plants which either do not occur today, or are of very limited geographic extent. A similar phenomena [sic] can be observed in the Pleistocene record of terrestrial and freshwater mollusks (Taylor 1965; Miller 1975), amphibians and reptiles (Holman 1976; Van Devender et al. 1977), and small mammals (Lundelius 1967; Graham and Semken 1976). All of these groups provide examples of the Pleistocene sympatry of forms which are now allopatric by great distances. This phenomena [sic] has been interpreted as the result of less seasonality with the reduction of summer temperatures and the rise of winter temperatures. In most

> cases we might also expect moisture to fall more uniformly throughout the year and summer moisture to be more effective. A reduction of seasonality would permit the expansion of the ranges of many organisms as it is often the environmental extremes of heat, cold, aridity, etc., rather than the yearly averages which limit distributions. As the ranges expand, more of them would overlap and would thereby create communities of greater heterogeneity. It is this increased heterogeneity that gave the ice age communities their special 'flavor' and it is through the loss of this heterogeneity that we obtain our modern simplified and rather depauperate communities [Original references included].

The model summarized [above] by Martin et al. (1985) stands in sharp contrast to simpler, earlier models like the cliseral shift model (see Dansereau 1957; Martin 1958) which it has largely replaced in the literature. Data and discussions such as those presented thus far, or such as the excellent synthesis by Graham and Mead (1987), fully corroborate the major ecologically-generated predictions for equable Pleistocene climatic regimes. Together they point not only toward unique climatic and biotic regimes not duplicated in the Holocene but also toward increased ecological diversity. These observations also have significant consequences relative to the dynamics of Pleistocene ecosystems (cf. Guthrie 1984).

Guilday (1984:251), Martin et al. (1985:26), and Graham and Mead (1987:371-372), all speak of the richness of diversity of Pleistocene ecosystems. This consideration, however, appears to be only the first step in appraising Pleistocene ecosystems. Following a detailed review of the literature, Guthrie (1984:266) offers the following:

> What does this outstanding diversity of Pleistocene biotas mean? One problem is that our concepts of woodlands, grasslands, savanna, tundra, etc., based on current biomes, simply do not fit the Pleistocene. Webb (1977, 1978) proposed that the Pleistocene was characterized by "savanna" vegetation patterns. But the complex mixtures of biota present in Pleistocene deposits indicate more than savanna—they indicate a diverse and complex association of steppe species and a complex association of woody plants and even more complex mosaics of the two... Given a long and internally varied growth season, plant competition can result in temporal and phenological species displacement rather than spatial elimination. Thus a long and varied growth season on a productive substrate increases 'sympatric tolerance.' This diversity normally occurs in a moderate to fine-grain mosaic. The complex vegetative mosaic is then reflected in the animals: insect species emerge and reproduce in a sequential pattern, tracking flowering and seed production. The diversity is seen among birds and mammals which also experience more heterogeneous associations [Original references included. Emphasis in original].

On this basis Guthrie (1984:267) goes on to compare the environments occupied by large herbivorous mammals during the Pleistocene and the Holocene by means of a series of contrasts. The fine-grained mosaic environments of the Pleistocene (the environmental "plaids") would have favored habitat generalists, consuming mixed food diets, in conditions of higher local species diversity. By contrast, the biogeographically zonated environments of the Holocene (the environmental "stripes") would favor habitat specialists, subsisting on monocultural diets, in conditions of low local species diversity.

The contrasts between these two types of biotic systems are profound and significant. Guthrie (1984), and Graham and Mead (1987) go on to focus on the ecological shocks and readjustments which took place at the end of the Pleistocene and early Holocene as one biotic system disintegrated, ultimately leading to Holocene biotic communities and ecosystems. From a somewhat different perspective, it might be worth recalling, however, that these shifts took place over a relatively short period in the Quaternary. Furthermore, although we may well be familiar with Holocene conditions and somewhat less certain about Pleistocene environments, it should be pointed out that the rich, varied, and complex ecosystems of the Pleistocene were more typical of the Quaternary as a whole than the ecological conditions which have dominated world environments in the last ten thousand years.

However anomalous or atypical Pleistocene conditions may appear from a modern perspective, the sheer duration of Pleistocene conditions would have meant that Pleistocene ecosystems and the relationships and adaptations of component plants and animals would have had considerably more time to develop than those in Holocene ecosystems. The richness and complexity of Pleistocene ecosystems (and the potential stability of some of these complex interrelationships) would have been due not only to a more equable climate and a longer growing season, but also to the considerably longer periods of time available to develop such levels of ecological and adaptive complexity. One might well argue that it is Holocene, not Pleistocene conditions which should be viewed as anomalous.

Under such prevailing Pleistocene conditions, numerous and unique local microhabitats (each containing distinctive combinations of plants and animals) would have had ample opportunities to develop. Such microhabitats would not have been anomalous for Pleistocene conditions; but rather, they would have represented local expressions of the broader trend toward the development of a unique and complex mosaic of environments.

Finally, beyond these general patterns, Pleistocene plant and animal distributions would have exhibited long-term and broad scale variations both in time and space. Long-term variations over spans of thousands of years in insolation values along with more complex Earth-bound climatic responses would have produced large-scale fluctuations in biotic responses superimposed upon the more generalized patterns described thus far.

Likewise, large-scale regional differences would have existed due to topographic variables. Most of these would have been largely time-independent, though some regional patterns (such as major shifts in air circulation patterns due to the location and size of glacial masses) would have been more limited temporally.

Some authors (e.g., Graham 1979; Martin et al. 1985; and Graham and Mead 1987) have provided discussions aimed at defining such large-scale time- and space-bound Pleistocene regional biotic patterns based on paleoenvironmental, rather than modern environmental criteria. Unfortunately, detailed discussion of these proposals are clearly beyond the scope of the present discussion.

4.4.3 MID TO LATE WISCONSINAN PALEOENVIRONMENTAL PARAMETERS

The chronological calculations reviewed earlier indicated that the initial entry of human groups into the New World most reasonably lies sometime during the Mid-Wisconsinan. The purpose of this section, then, is to address specific paleoenvironmental issues which would have influenced these population movements.

One of the principal components in establishing a chronological perspective on paleoclimatic and paleoenvironmental conditions centers on the influence exercised by past insolation patterns on Earth's climates. The following brief summary is based on the data presented by Berger (1978:145).

Insolation values between 30 and 60 degrees north latitude remained remarkably constant and near typical average values for the Pleistocene for 35 thousand years between 65,000 and 30,000 years ago. Major shifts in seasonal insolation values took place between 80,000 and 65,000 years ago; and from 30,000 years ago to the present.

The periods approximately between 77,000 and 66,000 years ago, and between 28,000 and 19,000 years ago were characterized by having anomalously higher insolation values in the winter and lower insolation values during the summer. The reverse of this seasonal insolation pattern was most extremely expressed in the interval between 18,000 and 4,000 years ago.

It is important to emphasize that these observations express insolation patterns and not climate or temperature. Furthermore, some Earth-bound systems tend to lag behind variations in insolation patterns (i.e., "orbital forcing") by perhaps up to 4,000 or 4,500 years (see Ruddiman et al. 1980:38). Nevertheless, one study by Heusser et al. (1985) seems to suggest that there is a much closer and more direct relationship between variations in insolation rates and both temperature and precipitation than the relationship between insolation rates and global glacial mass.

In order to assess the extent of continental glaciation we must turn to the marine oxygen isotope record. As Bryson (1985:275) has remarked,

> ...a couple of decades ago it was thought that oxygen isotope ratios in marine organisms might directly reflect the ambient temperature per se through the temperature dependence of the fractionation rate. This required the assumption of a constant isotope ratio in the sea. Now it appears that this assumption is incorrect at the glacial time scale.

> The average oceanic oxygen isotope ratio is now thought of primarily as a measure of continental ice volume [Emphasis in original].

On this basis, using the data presented by Martinson et al. (1987:20-21) and the data summarized in Figure 4.2, we can see that beginning around 75,000 years ago oxygen isotope values dipped below 0.00; and continued well below this value until about 11,500 years ago. Oxygen isotope values stabilized around a mean value of - 0.43 between 70,000 and 15,000 years ago.

Significant departures from this mean value took place between 60,000 and 45,000 years ago with high values reaching -0.16 around both 55,000 and 50,000 years ago. Other significant departures from this mean value took place beginning 22,000 years ago when values reached - 0.79, climbing to - 0.62 around 15,000, - 0.06 around 12,000, + 0.31 around 10,000, + 0.86 around 4,200, and + 0.92 around 200 years ago respectively.

If we gauge this pattern by comparing these values to estimated continental ice mass volumes of the decaying ice sheets toward the end of the Pleistocene (around 11,500 years ago), it becomes obvious that the average oxygen isotope value of - 0.43 (between 70,000 and 15,000 years ago) represents a considerable increase in continental glacial ice mass worldwide over their terminal Pleistocene values. This increase in ice mass should have been correlated with a corresponding lowering of sea level in the world's oceans.

A major decrease in ice mass (with a concomitant rise in sea level) would have taken place between 60,000 and 45,000 years ago. By contrast, the major increase in ice mass (and further lowering of sea level) is represented by the full-glacial conditions between 25,000 and about 14,000 years ago. Needless to say, these projections apply for continental ice masses worldwide; and thus can not be used as direct gauges of the size and geographical extent of specific ice sheets at specific points in time. Therefore, we must discuss the details and chronology of North American glaciation separately from the history of eustatic changes in worldwide sea levels.

The North American record of Mid-Wisconsinan glaciation seems to correlate well with the projections summarized above. Although the advance of ice during the Late Wisconsinan resulted in the destruction of much of the Mid-Wisconsinan record, the data which remain tend to support the major outlines as inferred from the oxygen isotope record.

In a sequence of publications Knut Fladmark (1978, 1979) presented paleogeographical and paleoenvironmental data intended to support the possibility of a coastal route of entry for man into the Americas. Later, in 1983, he published a survey of much broader scope encompassing the whole of North America during the Mid to Late Wisconsinan. This comprehensive and thorough account represents an excellent source of detailed information which need not be repeated. Specific components in his treatment of the timing and size of glacial mass and specific paleoenvironmental conditions were supported by Warner et al. (1984), Andrews (1987), Porter (1989), Mathewes (1989), and Schweger (1989).

Based on all these sources, we may observe that following a major glacial advance around 70,000 years ago, the Mid-Wisconsinan Laurentide ice sheet occupied a much reduced area confined to the eastern one-third (Fladmark 1983:17) or eastern one-quarter (Andrews 1987:18) of Canada until the onset of full-glacial conditions during the Late Wisconsinan. During this same period the Cordilleran and Beringian lowlands appear to have remained unglaciated (Fladmark 1983:16; Andrews 1987:17-18; Porter 1989:250) with local glaciation restricted to the higher elevations. These, then, represent typical Mid-Wisconsinan glacial conditions which would have only occasionally been broken by one or more short-lived glacial readvances sometime between 30,000 and 45,000 years ago (Fladmark 1983:16).

> The advance phase of Late Wisconsin glaciation saw diminishing ice-free areas both east and west of the Rocky mountains at 25-18,000 B.P. Cordilleran glaciers and some Laurentide lobes climaxed at about 14,500 B.P., several thousand years after the usually cited main southern Laurentide maximum at circa 17,000 B.P. (Fladmark 1983:21).

The dynamic, and often localized movement and culmination of glacial advance during the Late Wisconsinan is well documented by Andrews (1987); while the irregularities in the timing of local glacial advances from the two ice sheets near the ice-free corridor led Rutter (1980:7) to conclude that

> ...it is becoming apparent that there was only a short period of time when Laurentide-Cordilleran ice could have coalesced in Wisconsin time, or during the last 100,000 - 120,000 years. This could have been in Early Wisconsin time, which we know little about, and in Late Wisconsin time in the northwestern part of the continent for a maximum of about 10,000 years; or put another way, a maximum of about 10,000 years in the past 50-70,000 years (the lower limit of the middle-Wisconsin).

Still another component appears to be critical in understanding the paleoenvironmental parameters which may have influenced the distribution of plants, animals, and human populations over the last 70,000 years. Just as an understanding of the growth and decline of past continental ice sheets is significant, so too, the eustatic fluctuation of worldwide sea levels become important contributors to such biogeographical reconstructions. Both factors, determining the availability of a direct land route between the Soviet Far East and Alaska, and the emergence of the continental shelf at different times over the last 70,000 years, constitute critical components which need to be addressed.

Figures 4.3a through 4.3e give several views of the modern North Pacific basin, with particular emphasis on the bordering land masses, including continents and islands, and the current 100 meter contour line below sea level. These maps represent present land forms—a

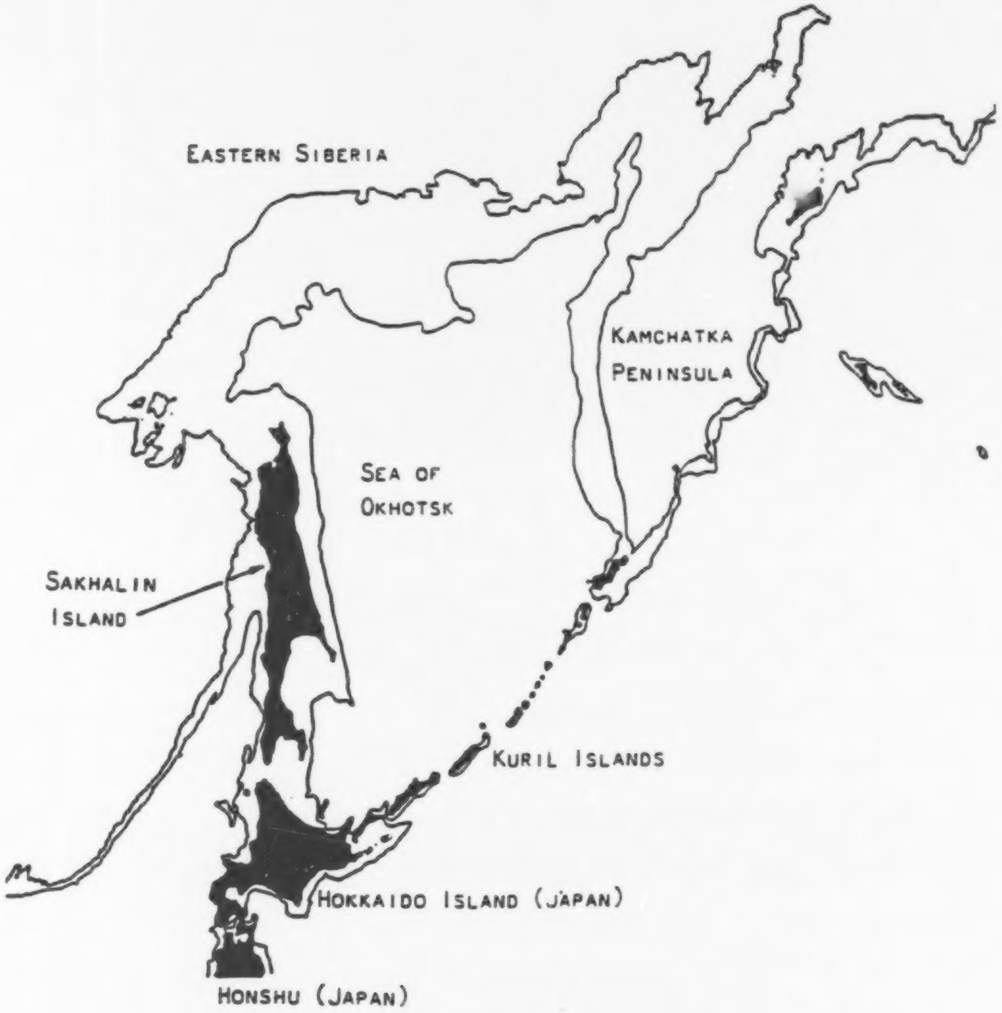
EASTERN SIBERIA
KAMCHATKA PENINSULA
SEA OF OKHOTSK
SAKHALIN ISLAND
KURIL ISLANDS
HOKKAIDO ISLAND (JAPAN)
HONSHU (JAPAN)

Figure 4.3a

North Pacific Coastal Topography

Figures 4.3a through 4.3e represent modern shoreline topography of the North Pacific basin, including the 100 meter contour line below present sea level. Modern islands are shown in solid black. These five composite maps were redrawn from maps in Hopkins (1959), Rand McNally (1965), and the Institute of Marine Resources (1970).

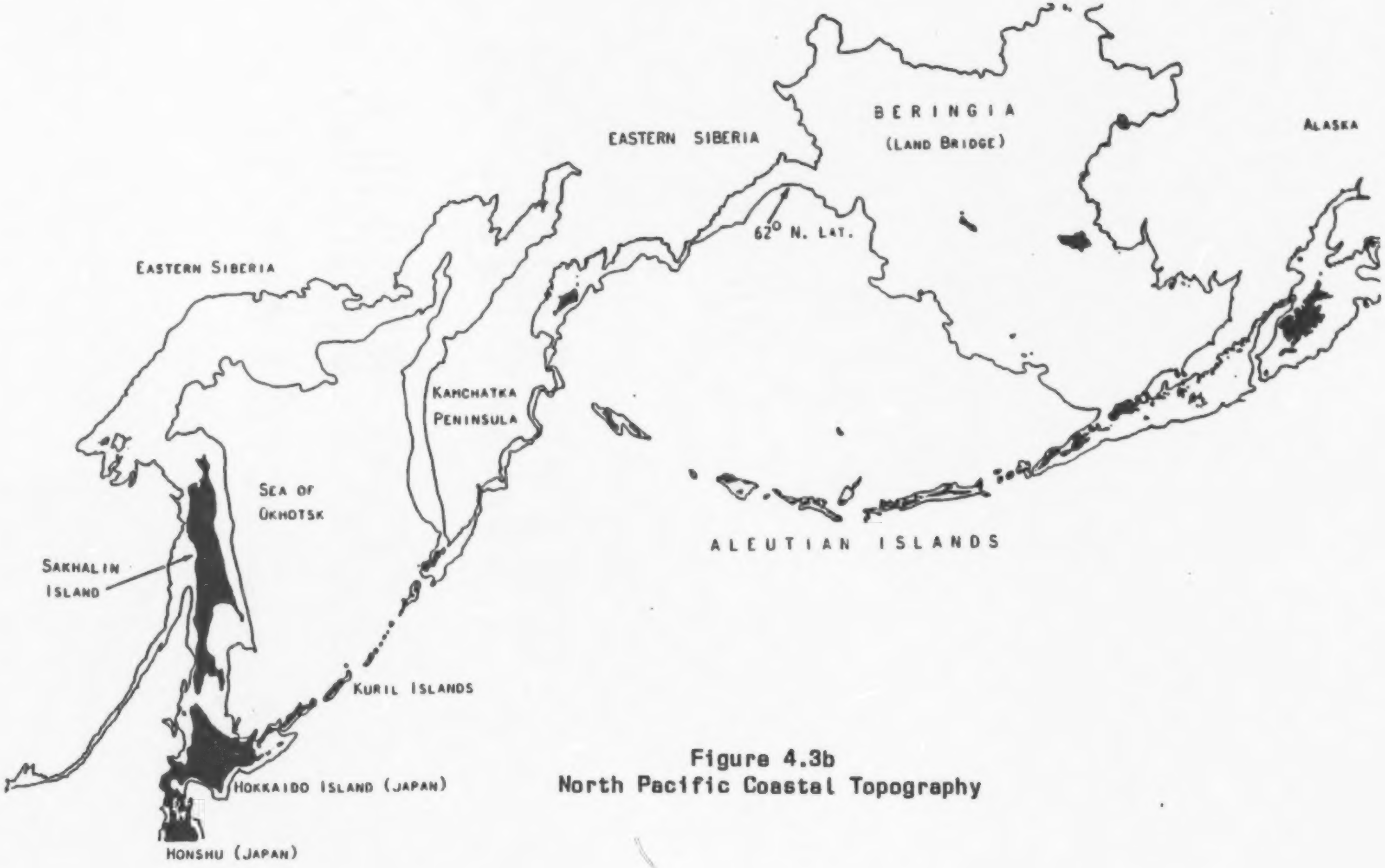
EASTERN SIBERIA
BERINGIA
(LAND BRIDGE)
ALASKA
62° N. LAT.
EASTERN SIBERIA
KAMCHATKA
PENINSULA
SEA OF
OKHOTSK
SAKHALIN
ISLAND
ALEUTIAN ISLANDS
KURIL ISLANDS
HOKKAIDO ISLAND (JAPAN)
HONSHU (JAPAN)

Figure 4.3b
North Pacific Coastal Topography

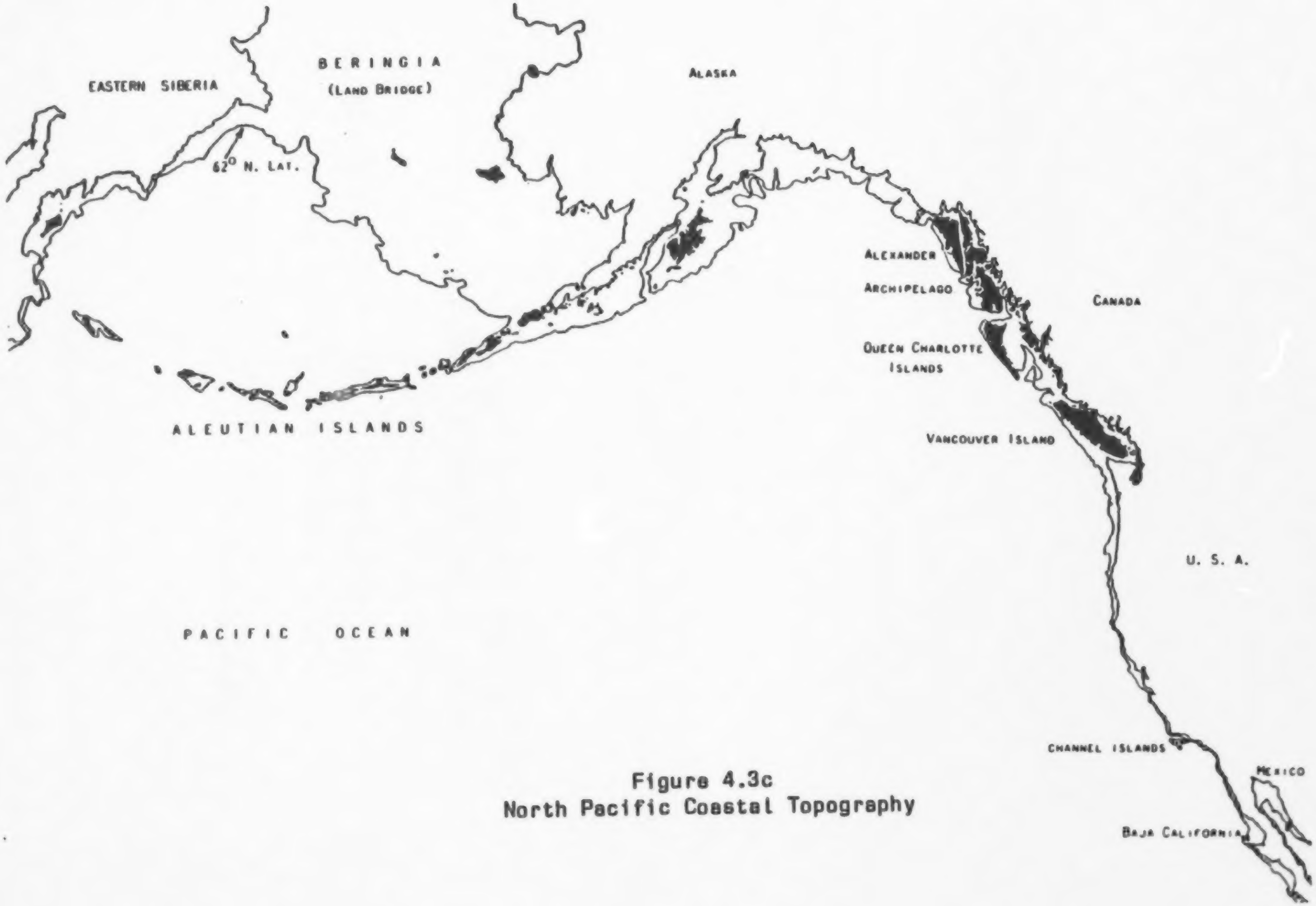
EASTERN SIBERIA
BERINGIA
(LAND BRIDGE)
62° N. LAT.
ALASKA
ALEXANDER
ARCHIPELAGO
CANADA
QUEEN CHARLOTTE
ISLANDS
VANCOUVER ISLAND
ALEUTIAN ISLANDS
PACIFIC OCEAN
U. S. A.
CHANNEL ISLANDS
MEXICO
BAJA CALIFORNIA

Figure 4.3c
North Pacific Coastal Topography

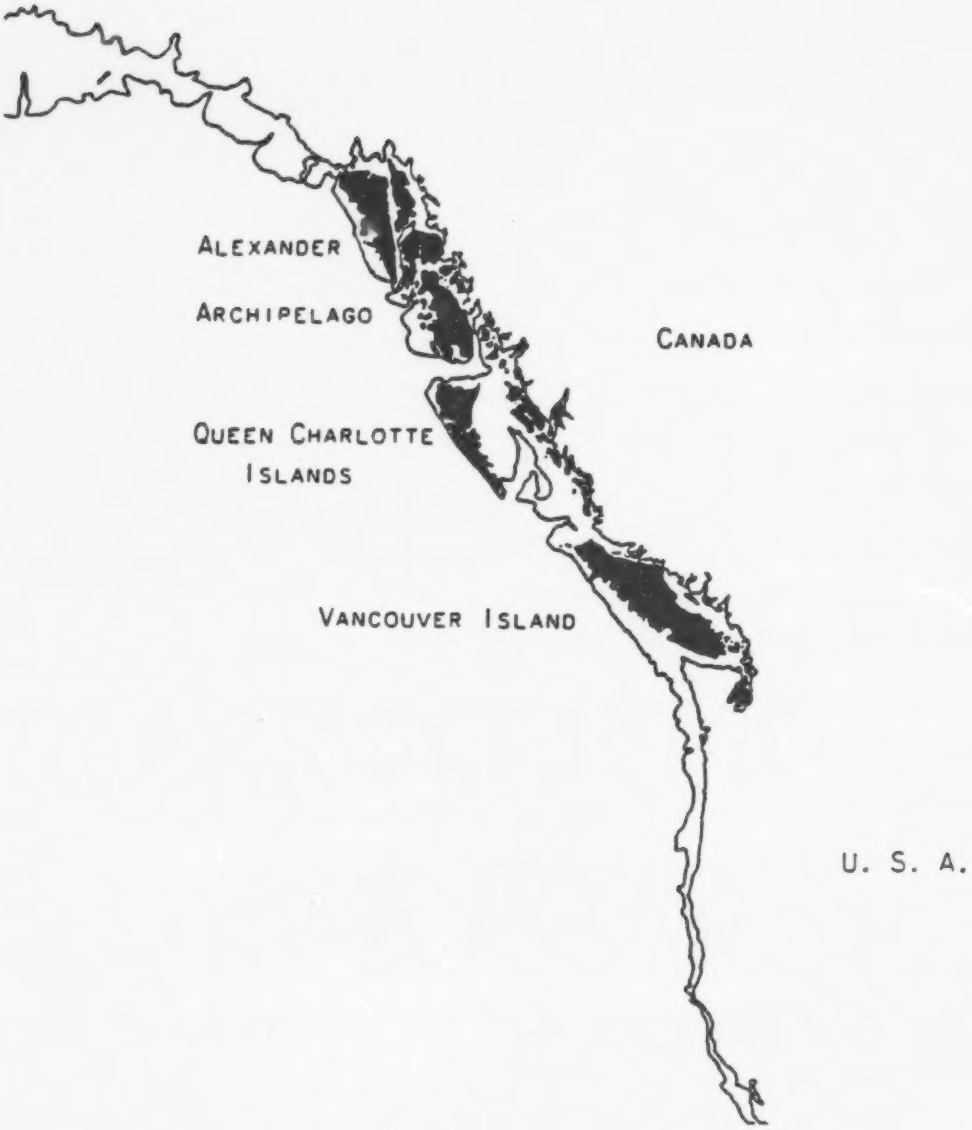
ALEXANDER
ARCHIPELAGO
CANADA
QUEEN CHARLOTTE
ISLANDS
VANCOUVER ISLAND
U. S. A.

Figure 4.3d
North Pacific Coastal Topography

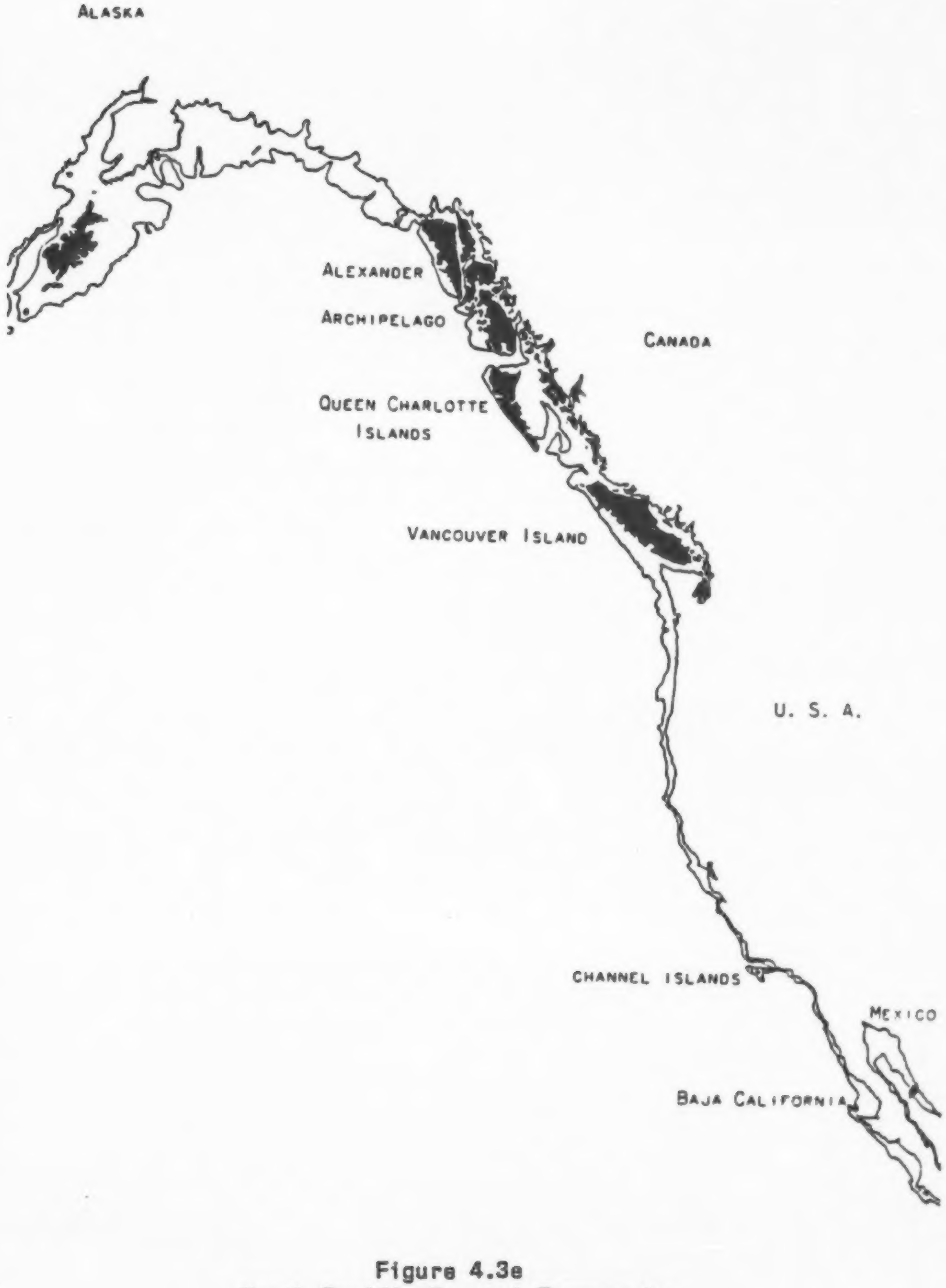
ALASKA
ALEXANDER
ARCHIPELAGO
CANADA
QUEEN CHARLOTTE
ISLANDS
VANCOUVER ISLAND
U. S. A.
CHANNEL ISLANDS
MEXICO
BAJA CALIFORNIA

Figure 4.3e
North Pacific Coastal Topography

point of departure for further discussions—and do not take into account differences due to local tectonic and isostatic movements which took place during the last 70,000 years.

Given current topography and climate,

> The Bering Strait separates the Chukchi Peninsula of northeast Siberia from the Seward Peninsula of Alaska; it is 55 miles wide and has a maximum depth of 170 feet. Like the northern parts of the Sea, it freezes from October to June (Huxley 1962:69).
>
> The Bering-Chukchi platform is monotonously flat. St. Lawrence Island, St. Matthew Island, the Pribilof and Diomede islands, and several smaller islands near the coast of Seward Peninsula are the only prominent topographic highs in the Bering Sea and Bering Strait. Herald Shoal, 42 feet deep, lies in the central Chukchi Sea. Aside from these, the surface of the platform is devoid of sharply defined topographic features. Bottom gradients are so small that they are difficult to measure (Hopkins 1959:1519).

Under these topographic conditions it is obvious that any significant lowering of sea levels would result in a narrowing of the Bering Straits and an exposure of portions of the Bering-Chukchi platform. A drop of about 36 meters below modern sea level would narrow the gap to a channel 32 kilometers (20 miles) wide with a maximum depth of 15 meters (Hopkins 1959:1524).

It is likely that such a drop in sea level would result in more extensive winter freezing of this channel—especially in its shallower portions. At slightly more than 50 meters below current sea level the two continents would be joined by land, while a further drop to about 91 meters below modern sea level

> ...would result in the exposure of nearly all the Bering-Chukchi platform, and Alaska and Siberia would be joined by an almost featureless plain extending nearly 1000 miles from the north shore of a shrunken Bering Sea to the south shore of the Arctic Ocean (Hopkins 1959:1524-1525).

Other land masses—most importantly the continental shelf areas and shallow coastal areas surrounding islands—would also be exposed due to varying drops in sea level (see Fladmark 1978, 1979, 1983, and Figures 4.3a through 4.3e). The effects of exposing such land masses would have included the shifting of shorelines, increases in total land area and size of islands, the joining of modern islands to each other or to continental areas, the reduction of maritime distances between some islands and continental land masses, and the occasional formation of continental land bridges. Naturally, these effects would have been proportional to the relative amount of sea level lowering at each location at specific times; and thus can only be related in a general sense to absolute values of past worldwide sea levels.

It is difficult to obtain exact absolute values for sea levels in the past, since all measurements are drawn from local samples which may also reflect local tectonic changes and our capacity to allow accurately for or quantify such local intervening variables. Oxygen isotope records provide the most uniform diachronic source of information for worldwide changes in sea level, but they also must be calibrated by reference to geological field observations. Having thus acknowledged some of the inherent difficulties in any reconstruction of past worldwide sea levels, we may proceed to summarize briefly the evidence for worldwide eustatic sea level changes over the last 70,000 years.

Beginning with the oxygen isotope record as presented by Mix (1987:126), we can approximate the sequence of major worldwide sea level changes as follows. Beginning between 85,000 and 70,000 years ago, sea levels dropped from a high of about -12 meters below present levels to about -47 meters around 75,000 years ago, and down to around -90 meters 70,000 years ago, respectively. This drop would have exposed most of the Beringian land mass, and compared to modern underwater topography the altered shoreline would have mimicked closely the additional contours presented in Figures 4.3a through 4.3e.

Between 70,000 and 30,000 years ago worldwide sea level values fluctuated between highs of -25 meters and lows of -75 meters below current level. Following the low value of -90 meters 70,000 years ago, sea level rose to -42 meters by 66,000 years ago, to a high of -25 meters by around 62,000 years ago.

Sea levels continued to fluctuate between -25 and -35 meters below modern sea level until around 58,000 years ago, when sea levels began to drop significantly, attaining low values of about -75 meters 55,000 to 51,000 years ago. This drop represents the lowest worldwide sea level value for the Mid-Wisconsinan, between about 70,000 and 30,000 years ago. At this level Alaska and Siberia would have been joined by a significant land mass, although this value is below the size of the land mass exposed between 85,000 and 70,000 years ago, and it is well below the land area exposed during the Wisconsinan maximum.

Around 50,000 years B.P. sea level rose rapidly to -49 meters. Between 50,000 and 30,000 years ago; additional fluctuations in sea level ranged between high values of about -40 to -43 meters 45,000, 40,000, and 30,000 years ago, and lows of -60 meters, and -70 meters around 42,000 and 33,000 years ago respectively. Relative to the modern topography of the Bering Strait, this change would mean that the actual span of water between continents would have been reduced to less than 32 kilometers throughout this period, and more than once (around 42,000 and 33,000 years ago) a land bridge would have been formed.

Following a high sea level value of around -43 meters 30,000 years ago, sea levels dropped to their lowest values in the Wisconsinan, reaching about -143 meters below modern sea level between 20,000 and 15,000 years ago. After 15,000 years ago sea levels began to rise rapidly, so that by 10,000 years ago worldwide sea levels stood around -28 meters below present sea level.

As I have tried to point out earlier, there is no absolute means of ascertaining exact values for worldwide sea level in the past. Therefore, it is important to temper these calculated sea level values by comparing them to sea level values established by other researchers in different areas. I have attempted to do so, drawing on the published values given in Hopkins (1959, 1982); Cann et al. (1988); Fladmark (1979, 1983); Booth (1987); and Kutzbach (1987).

Years B.P.	As based on Mix (1987)	Corrected Values
85,000	- 12 m	- 9 m
75,000	- 47 m	- 36 m
70,000	(- 90 m) *	(- 69 m) *
66,000	- 42 m	- 32 m
62,000	- 25 m	- 19 m
62 to 58,000	- 25 to -35 m	- 19 to - 27 m
55 to 51,000	(- 75 m) *	(- 58 m) *
50,000	- 49 m	- 38 m
45,000	- 40 m	- 31 m
42,000	(- 60 m) *	- 46 m
40,000	- 40 m	- 31 m
33,000	(- 70 m) *	(- 54 m) *
30,000	- 43 m	- 33 m
20 to 15,000	(- 143 m) *	(- 110 m) *
10,000	- 28 m	- 22 m

Table 4.5
Mid to Late Wisconsinan Sea Levels

Discussion and references given in text. Corrected values plus thirty percent equal values based on Mix (1987:126).
* Asterisked numbers represent sea levels below current topographic level required to create a land bridge between Asia and America.

Although the values for sea level at various times differ among these sources as well, it seems that the sea level values given earlier (based on the oxygen isotope data) may have been exaggerated by approximately thirty percent of their actual values. This difference seems to be a reasonable and conservative adjustment of the figures given above. Table 4.5 compares and summarizes both sets of values for past sea levels.

These sea level values (including corrected values) can be interpreted to mean that throughout the Mid-Wisconsinan (with only a few exceptions) lowered sea levels would have posed little or no barrier to movement between Asia and America. During this time, it is likely that a land bridge existed between the continents at least three times, including around 70,000, 55 to 51,000, and 33,000 years ago. Additionally, based on the original sea level values obtained from Mix (1987), there would have been a land bridge present around 42,000 years ago.

Corrected values for this time period (see Table 4.5 for 42,000 years B.P.) show a maximum depth of the Bering Straits (based on modern sea floor topography) of 5 meters. It is most likely that such a shallow and constricted channel would have frozen easily; and would not have posed a major barrier, either.

Although it is impossible to reconstruct exactly the location of ancient shorelines over such vast areas as the North Pacific throughout the Mid-Wisconsinan, it is safe to say that the lowering of sea levels would have opened up vast new coastal areas beyond modern shores, while effectively decreasing the distances between islands, and between islands and continental land masses. Depending upon the specific amount of sea level lowering, these changes might have been relatively minor, to quite significant, approaching though never quite reaching the 100 meter contour (shown in Figures 4.3a through 4.3e) until the Late Wisconsinan.

Furthermore these coastal areas, as Fladmark (1978, 1979, 1983) has pointed out, would have continued to offer milder climates than inland areas even in the face of temporal variations in climate, while supporting a highly productive ecotonal zone. The moderating influence of oceans on near-shore environments is well known and fairly well understood.

The degree to which moderating influences would have yielded acceptable environments for plants, animals, and human populations in the Mid to Late Wisconsinan would have been significantly different over time. This point is clear from Fladmark's work, whose earlier publications (1978, 1979) focused on Late Wisconsinan conditions, while in 1983 he extended his observations to include the Mid-Wisconsinan. Likewise, moderating influences would have varied (as they do today) by latitude, and by the operation of a number of local factors such as topography and ocean currents.

In his publications Fladmark focused attention on the persistence of glacial refugia along coastal areas in the Late Wisconsinan. Refugia are inferred from biotic data pointing to the presence of numerous unique species whose evolution along coastal insular areas was due to glacial isolation in the Late Wisconsinan. These inferences, however, are not universally accepted.

Ritchie (1987:109-110) acknowledges the existence of unglaciated areas supporting grass, sedge, and willow on the Queen Charlotte Islands between 16,000 and 14,000 years ago; but he rejects the argument of a Late Wisconsinan refugium on the basis of modern floral distributions. By contrast, other authors (Mathewes and Anderson 1989:486; Mathewes 1989:491) tend to favor the possibility of a refugium, presenting detailed field data in support, while acknowledging the incomplete nature of the available evidence for a Late Wisconsinan refugium thus far. These difficulties in interpretation are particularly troublesome in view of the potential blockage by ice of the Pacific coastal area of the Alaska Peninsula between the Beringian coast and Kodiak Island during the Late Pleistocene (Fladmark 1983:26).

The uncertainties regarding the existence of a Late Wisconsinan refugium on the Queen Charlotte Islands can be contrasted with our understanding of Mid-Wisconsinan conditions. In the same article, Mathewes (1989:488) reports a bed of peat at the Pilot Mine site of eastern Graham Island. He reports that this "...buried peat under a till is dated at between 45 700 ± 970 BP (GSC-3534-2) and 27 500 ± 400 BP (GSC-3530)" (Ibid, see also Warner et al. 1984). Clearly conditions along the Pacific margin of North America were considerably different in the Mid-Wisconsinan as compared with the Late Wisconsinan.

Contrasts between Mid and Late Wisconsinan conditions can also be found for the eastern foothills region of the Rocky Mountains; while reconstructions of the glacial and biotic history of the "ice-free corridor" have undergone major modifications in the last decade. The perspective which held that glacial ice coalesced, forming a physical barrier to population movements for thousands of years, has gradually been abandoned (see for example Rutter 1980; Fladmark 1983:28). More recently Schweger (1989:498) has summarized our knowledge of Late Wisconsinan conditions in this region as follows:

> From studies performed to date, all that is known is that between 24 and 11.4 ka cold, arid tundra vegetation existed in the ice-free corridor from the International Boundary to Athabasca River valley. Woolly mammoth may have been present, lemmings certainly were, perhaps along with horse, caribou, bison, camel, and sheep. At 15.5 ka the botanical record marks the start of deglaciation and by 11.4 ka...pioneering forests of birch and poplar or aspen were established in the Foothills region of the corridor.

Throughout much of the Mid-Wisconsinan the Laurentide ice sheet was restricted largely to the eastern third or quarter of Canada, while the Cordilleran area was limited to moderate quantities of mountain glaciation at higher elevations (see Andrews 1987; Porter 1989; Fladmark 1983). Insolation values indicate that temperatures were probably more equable and perhaps somewhat cooler than today.

The flora and fauna of the "ice-free corridor" region during the Mid-Wisconsinan also reflect these differences. By sharp contrast to biotic conditions during the Late Wisconsinan, data from several sites in north-central Alberta (such as the exposure at Watino dated between 43.5 to 27.4 ka) leads Schweger (1989:496) to conclude that the "...Middle Wisconsinan interval [was] characterized by open vegetation with scattered conifers and an invertebrate fauna similar to that of present-day central Alberta." This interpretation is clearly consistent with expectations based on glacial and insolation data.

Farther west, conditions along the northwestern Pacific margins also suggest significant contrasts between the Mid and Late Wisconsinan. Drawing on pollen samples and data from radiolaria, Heusser and Morley (1985) have summarized a sequence of projected paleoenvironmental conditions for Japan during the Wisconsinan. Following a relatively cool and moist interval between 80,000 to 60,000 years B. P., climates shifted to variably cool and drier conditions between 60,000 and 25,000 years B. P. (Heusser and Morley 1985:67).

> Fluctuations in pollen from boreal conifer forests (Picea, Abies), forest-tundra transition (Betula, Alnus, S. selaginoides), and the intermittent presence of cool temperate associates (Cryptomeria) during this period (80,000-25,000 yr B.P.) suggest changing precipitation and temperatures in an overall subarctic regime in northeastern Hokkaido and boreal environments in northeastern Honshu (Heusser and Morley 1985:67-68).

Heusser and Morley (1985) contrast these Mid-Wisconsinan conditions to Late Wisconsinan conditions by stating that

> During the last glacial maximum (25,000-18,000 yr B.P.) pollen from Picea-dominated conifers and bog plants (including Cyperaceae and Sphagnum) indicate boreal and subarctic conditions. The presence of S. selaginoides, an arctic-alpine indicator species...suggests tundra or park-tundra climate in northern Hokkaido (Heusser and Morley 1985:67).
>
> Between 18,000 and 10,000 yr B.P., Betula, Alnus, and conifer pollen are prominent. Abundant Pinus and virtual absence of Cryptomeria reflect dry, gradually warming climate during the late-glacial transition (Ibid.).

These reconstructions of Late Wisconsinan conditions receive further detailed support in the work of Tsukada (1985).

Significant differences existed in the inland and coastal records of Japan in both the Mid and Late Wisconsinan. A large temperature depression recorded at inland sites dating between 60,000 and 50,000 years ago is absent in the data from coastal sites, reflecting differences in oceanic-dominated as opposed to more continental climates (Heusser and Morley 1985:68).

These differences are similarly documented for the full-glacial conditions of the Late Wisconsinan in Japan. According to Tsukada (1985:378), during full-glacial times a narrow strip (less than 50 kilometers wide) of mixed temperate conifer and deciduous broadleaf forest fringed most of the east coast of Honshu, grading rapidly inland into a boreal conifer forest. Only the northern fourth of Honshu and Hokkaido lacked this fringe.

Coastal conditions farther north along eastern Kamchatka and approaching western Beringia are much more difficult to reconstruct. V.

P. Grichuk (1984) provides generalized vegetation maps which should be interpreted more as projections rather than as actual sources of detailed information, due to admitted problems in dating and limited sampling.

Keeping these caveats in mind, Grichuk projects full-glacial (Late Valdai) conditions in far northeast Asia as follows:

> The zones in the periglacial region have limited and rather distant analogues in the modern vegetation. This region was characterized by widespread steppe and forest-steppe vegetation and polar deserts and by the near absence of forest vegetation. Polar deserts are reconstructed in parts of eastern Arctic Siberia and on islands in the East Siberian Sea; this suggests that the exposed Arctic Ocean Shelf was entirely covered by this type of vegetation. High-mountain deserts and tundras are reconstructed for the mountains of central and eastern Siberia (Grichuk 1984:177).

Grichuk's projections of Late Wisconsinan conditions might be tempered by two observations derived from Beringia to the northeast of this area. By contrast to Late Wisconsinan full-glacial conditions, Anderson's work (by the Arctic Circle) in northwestern Alaska points to significantly different conditions during the Mid-Wisconsinan. She states that pollen samples containing

> high Betula percentages and PAR...suggest that mesic conditions were relatively widespread. Prior to 27,000 yr B.P., conditions may have been sufficiently warm and moist to support small populations of Populus (Anderson 1988:270).

Given these observations, it might not be unreasonable to suggest that during the Mid-Wisconsinan, improved conditions may have also characterized the Pacific coast of Kamchatka over 700 kilometers farther south.

The other observation from Beringian data concerns relative contrasts between coastal and inland conditions. In an assessment of climatic and vegetation differences during the Late Wisconsinan, Young (1982) divided Beringia into five geographical areas. Anderson's (1988) data describes what Young (1982:190) has labeled "central Beringia." During full-glacial conditions of the Late Wisconsinan Young projects that

> the vast interior plains of central Beringia probably supported a rather sparse, open vegetation as the dominant type. The vegetation may have been intermediate between what we now think of as fell field and a more steppe-like character (Young 1982:190).

By contrast to this characterization of "central Beringia," "south coastal Beringia" is described as follows:

> South coastal Beringia must have been similar in many re-

> spects to western coastal Alaska of today, but it was probably subject to more extensive disturbance due to constantly shifting shorelines and the formation of very extensive estuarine and delta systems. I see no reason to suggest that the vegetation of the area was much different than that of coastal regions today. Dune fields must have been extensive; their vegetation would have been dominated by coarse grasses, mainly *Elymus arenarius*, as they are at present. Deltas and more stable flats probably supported a closed sedge-meadow vegetation, similar to that of the coastal portions of the Yukon-Kuskokwim Delta of today. Back shores and depressions between dunes and beach ridges would have supported a more varied community, including such species as several coastal Carices, *Salix fuscescens* and its relatives, *Atriplex gmelini*, *Stellaria humifusa*, *Cochlearea officinalis*, *Potentilla egedii*, *Angelica lucida*, and *Artemisia tilesii*. There must also have been extensive systems of fresh-water marshes, ponds, and slow-moving streams. These would have supported aquatic species such as various *Potamogeton* and *Sparganium* species, as well as emergent species such as *Hippurus vulgaris*, *Calla palustris*, and *Iris setosa* (Young 1982:187).

Although speculative, Young's work (above) serves to provide a vivid contrast between paleoenvironmental conditions along the coastal areas compared to the inland portions of Beringia during the Late Wisconsinan. Young's reconstruction of coastal conditions is further supported by the paleoclimatic projections of Barry (1982:200-201). Undoubtedly environmental conditions in a narrow strip paralleling the north Pacific margins between Japan and the northwest coastal areas of North America must have reflected the continued moderating influence of maritime climates both during Mid and Late Wisconsinan times. This conclusion mirrors and extends geographically the fundamental conditions discussed by Fladmark (1978, 1979, 1983) which prompted him to propose a coastal route of entry by man into the New World even during the Wisconsinan maximum.

4.4.4 THE TERMINAL PLEISTOCENE-EARLY HOLOCENE TRANSITION

The terminal Pleistocene marks a period of rapid and severe changes in most of the conditions discussed so far. In a relatively short period of time (between about 22,000 and 11,000 years ago) insolation patterns between 30 and 60 degrees north latitude shifted from high winter/low summer insolation (equable conditions) to high summer/low winter insolation (Berger 1978:145). The bulk of this shift took place between 20,000 and 15,000 years ago, with high summer insolation and low winter insolation trends reaching their extreme values around 10,000 years ago (see also Figure 4.2 for deviations in insolation from mean values). Summer-winter seasonal contrasts in insolation became extreme, while latitudinal zonation also became pronounced.

Allowing for the nearly 5,000 year lag of glacial systems behind changes in insolation patterns, the marine oxygen isotope record (as summarized in Figure 4.2) points to a rapid decline in world glacial mass. The decline in glacial mass caused the worldwide sea level to rise between 15,000 and 10,000 years ago by close to 100 meters over full-glacial levels (see Table 4.5).

Long-standing, typical Pleistocene ecosystems containing floral and faunal combinations which by modern standards appear disharmonious could no longer be sustained. Climatic changes at specific locales meant that niche tolerances of some forms were exceeded. As Graham and Mead (1987:396-397) put it,

> Climatic and environmental fluctuations at the end of the Pleistocene (ca. 10 ka) were significantly greater than those of the late Pleistocene or entire Holocene. Milankovitch forcing suggests that this is a time of maximum seasonal contrast... Faunal evidence supports such a contention and indicates change from a cold equable Pleistocene climate to a warmer more continental one. The magnitude of these changes is reflected in the response of the vertebrate communities. Enhancement of seasonal extremes (colder January and warmer July) caused species to migrate along environmental gradients in different directions, at different times, and with different rates. Modern community patterns began to emerge as the disharmonious biotas of the late Pleistocene were individualistically disassembled. Extinction of 32 genera of large mammals in North America may be one of the major consequences of this environmental change.

Although large-scale and rapid changes in biota appear to be concentrated near the Pleistocene-Holocene boundary, it is probably a mistake to lump these changes collectively into a single set of Earth-bound cause-and-effect relationships. Beyond the effects of orbital forcing on the Earth's climate, the dynamically interrelated physical and biotic systems of the Earth underwent a mosaic of unique and individualized responses and adaptive shifts which the passage of time and the size of our time scale units have obscured.

Guthrie (1984:260) proposed an explanation of some of these complex changes at the end of the Pleistocene, dividing discrete events into three headings:

> 1. Climatic changes in seasonal regimes decreased diversity, increased zonation of plant communities, and caused a shift in net antiherbivory defense strategies.
> 2. This change in the plant community resulted in a shorter, less diverse growing season for ungulates, decreasing net annual quality and quantity of resources available to many large mammal species.
> 3. These restrictions in available resources decreased local faunal diversity, body size, distributional ranges, and frequently resulted in extinctions.

Guthrie (1984) further supports this explanation with detailed discussions of the metabolic processes and digestive requirements of different classes of herbivores, relative to vegetational changes. His ingenious scheme shows how and why increased continental climates resulting in increased zonation would have favored the selective survival of dietary specialists capable of thriving on monocultural diets in relatively uniform environments of low plant diversity to the detriment of dietary generalists whose digestive and metabolic strategies required mixed food diets in a fine-grained mosaic environment of high species diversity. These differences would, in turn, point to selective differences between surviving, threatened, and extinct species.

Guthrie's explanation finds close support in the work of Graham and Mead (1987:387), while Guthrie's proposals represent an expansion of Graham's argument (1979:65), who stated that

> competition created by environmental changes would select for specialists that are ecologically limited to specific habitat types. Many of the nine surviving genera [of large Pleistocene herbivores] are restricted to one or two specific environments without much overlap. Therefore, survival of the nine large herbivore genera in North America is explained better by a climatic hypothesis rather than an overkill hypothesis.

Numerous near-synchronous extinctions can be viewed as vivid signs of crisis in biotic systems. Not surprisingly, titles of seminars and scholarly works frequently appear to focus on the phenomenon of extinctions. In North America, at least, the recovery of now-extinct species from archaeological contexts has further prompted consideration of the role played by man in bringing about extinctions. A thoughtful discussion of the strengths and weaknesses inherent in debates related to extinctions and their causal factors (including the potential impact of human hunters) has been provided by Grayson (1984).

Additionally, Grayson's (1987:289) elegant analysis of dated extinction of different taxa tends to support the conclusion

> ...that many of the genera that have yet to be dated to terminal Wisconsin times may not have survived beyond 12,000 yr B.P., and that the population numbers of many genera that did survive into the terminal Wisconsin may have significantly dwindled by then.

By itself, such a conclusion does not exclude the potential contribution of human hunting to the extinction of Pleistocene taxa, but it does tend to suggest a strong correlation with overall environmental changes which followed in the wake of the effects of orbital forcing on the Earth's systems.

Beyond a concern with extinctions, their timing, and relations to human activities, it is obvious that we are often still thinking in terms of a single phenomenon often explainable by single if not simple solutions. This consideration appears to be unfortunate, since an understanding of modern ecological systems and their constituent dynamics seems to be a more reasonable starting point. It is as if distance in time had rendered an otherwise enormously intricate and complex multidimensional process incapable of expression in any way except as a flat, two-dimensional picture. Given such simplistic explanations, it is precisely the shift away from this metaphorical lack of depth typical of many explanations—a shift which emerges in Guthrie's (1984) work—which is so inviting and attractive.

I believe that the key to understanding the biotic shifts which characterized the terminal Wisconsinan lies in recognizing and focusing on the widespread and dramatic crisis which had its origins in the external mechanics of orbital forcing on the whole spectrum of Earth-bound systems. Ultimate causality must begin here, even when immediate and intermediate causal effects for specific events will be traced to specific aspects of the dynamic interrelationships between Earth-bound systems—among which we must ultimately include our own species.

4.5 POPULATION ORIGINS - CONTINENTAL NORTHEAST ASIA

In an earlier segment of this chapter it was concluded on the basis of data from genetics, physical anthropology, and demography that the initial peopling of the Americas most probably took place sometime around 40,000 to 50,000 years ago. Since the ancestors of Native American populations originated in Asia, it follows that the first Americans must be derived from some Asiatic population living in northeast Asia just prior to the time of initial entry to the New World. Assuming the presence of some inland population in this region suggests the likelihood that representative sites from northeastern Siberia and the (former) Soviet Far East may already be known.

Mochanov (1978) provided a reasonable summary of twenty early sites and their chronology from this region. A number of the older sites were found in the Aldan region. Among these sites, the earliest period, the "Proto-Diuktai," was dated around 35,000 years ago. The "Proto-Diuktai" is followed by the "Diuktai," which begins around 18,000 years ago (Mochanov (1978:62, 64, 65). More recently, however, some of the earlier dates from the Aldan sites have been questioned. Yi and Clark (1983:198) note that

> The chronology and affinities of the so-called Dyuktai culture, the earliest Upper Paleolithic assemblage in northeastern Siberia are hotly debated. Geoarchaeological and chronometric data suggest that Dyuktai assemblages are no older than the middle part of the Lower Sartan glacial (that is, ca. 18,000 B.P.) and are substantially younger than is claimed in the Russian literature. Specifically, it appears that Mochanov's Dyuktai 1 (35,000-19,000 B.P.) is based neither on firm chronometric data nor on unambiguous stratigraphic relationships.

Some of the problems with the early dating of the Dyuktai culture are further explained in a discussion by Fagan (1990:195-196), who notes that

> Since Mochanov's excavations, however, the chronology of the Dyukhtai tradition has been thrown in doubt. Mochanov's radiocarbon dates from the Middle Aldan come from river terrace deposits that contain concentrations of animal bones and artifacts much disturbed by violent seasonal freezing and thawing cycles, such as are common in extreme arctic climates... The earliest securely dated Dyukhtai site is Verkhene-Troitskaya, also on the Aldan, which has been radiocarbon-dated to about 18,000 years ago.

Thus, although the earliest currently-known cultures from far northeast Asia may date from as early as 18,000 years ago, there is also a reasonable possibility that they may be dated as far back as 35,000 years ago.

Elsewhere, Derev'anko (1978:312) argues for the settlement of the Amur region (about 1,000 kilometers south of the Aldan) "...before the maximum Aldan (Riss) Glaciation in this area, some 300,000 to 200,000 years ago." According to Derev'anko

> The Amur basin lies about 1,500 kilometers from Choukoutien. But since the movement to the north was a slow one, primitive man could naturally cover such a distance only in the course of thousands of years, given favorable natural conditions. The earlier belief that Lower Paleolithic man inhabited chiefly tropical and subtropical regions is quite wrong. In the time of the early Peking man northern China had a colder climate than today.
>
> The fact that Peking man or other related species continued to live in northern China in the period of maximum ice testifies to great potentialities of our remote ancestors... It was evidently by chance and in small numbers that men penetrated to northern areas such as the Amur region [Ibid.].

Nearly 2,000 kilometers to the west, other sites in the Altai region of Siberia may lend further support to the early presence of man in northeast Asia.

> Numerous Mousterian localities also have been discovered northward in the Altai region of Siberia, principally in the Anui River drainage. Artifacts such as those from the multi-component Academician Okladnikov Cave include Levallois and radial cores, denticulates, Mousterian points, and several varieties of scrabloes (side scrapers). The uppermost level at the site has been radiocarbon dated to 33,300±520 yrs B.P. Basal Layer 7 of the cave may be of the Kazantsev (Sangamonian) Interglacial age (i.e., minimally 80,000 to 100,000 years ago).
>
> Most remarkable are the industries from the open-air

> stratified site of Ust' Karakol. Although rare at most other sites in the Altai, primary Levallois industries here include a significant component of fully bifacially flaked tools, some of which are elongated ovoid in outline. Charcoal derived from a middle level of this remarkable technocomplex has been radiocarbon dated to 31,410±1,160 yrs B.P. (Derev'anko 1989:4).

These, and other finds of Paleolithic age from the former Soviet Central Asia lend support to Derev'anko's claim (above) that the capacity to adapt to cold environments developed earlier than had been previously believed by some. Yet perhaps the degree to which these adaptations permitted the successful settlement of the far north was limited. These, and many other critical issues concerning the early prehistory of Siberia may be clarified among English speaking scholars as recent political changes in the former Soviet Union result in increased access to sites, and to an exchange of information through international symposia such as the recent symposia sponsored by the Russian Academy of Sciences at Novosibirsk.

Following a review of the archaeological data from Soviet Central Asia, Richard Davis states that

> ...the abundance and variety of Middle Paleolithic industries that show typological and technological overlap with many industries from various parts of Asia is a clear indication that Central Asia was not peripheral or marginal to the Paleolithic world. The message from Central Asia is that hunter-gatherers there were as central to the Paleolithic world as any other location in Asia. This does not mean, however, that all evolutionary events found elsewhere took place or were repeated in Central Asia. Indeed, for a number of stadial periods, Central Asia may have been largely uninhabited (Davis 1987:131) [emphasis added].

If Davis' analysis is correct, then the development of cultural adaptations which permitted the settlement of the Asiatic far north may have appeared relatively late, thus accounting for the revised chronology of the Dyuktai culture. Needless to say, under these circumstances it would be difficult to argue for a continental Siberian origin of peoples entering the New World between 40,000 to 50,000 years ago. Either the data or interpretations of the archaeology of continental far eastern Siberia is incomplete, or perhaps the initial peoples entering the New World originated elsewhere.

4.5.1 LATE PLEISTOCENE HUMAN ANTECEDENTS OF THE WEST PACIFIC RIM

In 1977 Australian archaeologist Sylvia Hallam published an article comparing the initial settlement of Australia and the Americas. She suggested that "...movements into the New World should be viewed in the wider context of subsistence, technology, and movement around the western littorals of the Pacific, resulting in the colonization not of one but of two new continents by men out of Asia" (Hallam 1977:128). Hallam's seminal article proposed five key points:

> (1) The earliness of watercraft is evidenced by chronology of the human diaspora through Wallacia and Greater Australia.
> (2) The simplistic nomenclature of chopper—flake traditions masks considerable complexity and technological potential, revealed in detailed Antipodean studies.
> (3) These traditions also have great potential for adapting to differing ecological zones, evidenced within Greater Australia; and for technological and economic innovation there, through Southeast Asia, and to Japan and the north Asian littoral.
> (4) The history of discovery and the nature of the evidence from Australia cannot validly be used to controvert early dates in the Americas.
> (5) Demographic data from Australia suggest that total commitment to a rapid—spread 'bow—wave' model for the peopling of new continents may be unwise (Ibid.).

Hallam's juxtaposition of Australian and New World origins is not unique in the literature. Others (notably Macintosh and Larnach 1976; Berger 1978; Shutler 1983; Thorne 1989; and Bednarik 1989) have also employed similar comparisons, though Hallam's approach seems to be among the earliest, most comprehensive, and rigorous in its approach. The intent of the final section of this chapter, then, is briefly to update the relevant data and to expand upon Hallam's construct, particularly on her first three points (above).

More specifically, given the focus on the initial peopling of the Americas, the most important components of Hallam's (1977) construct can be restated as four key proposals:

1. Modern Homo sapiens sapiens populations had either arrived or developed locally from earlier forms near the Pacific edge of East Asia prior to the peopling of Australia and the Americas.

2. Early East Asian Homo sapiens sapiens developed water craft and a maritime orientation during the Mid-Wisconsinan, as evidenced by the following:

a) A pattern of geographical spread along coastal areas which predates colonization of inland areas; or,

b) The recovery of archaeological or human fossil remains at locations along the southwestern Pacific rim which could only be reached by traversing significant stretches of open water.

3. The archaeological evidence from the northwestern Pacific littoral during the Mid-Wisconsinan further supports the existence of a maritime orientation northward along the Pacific rim.

4. The archaeological remains associated with these early maritime groups is both adequate and not contradictory to the general technological pattern associated with known simple, preliterate, ethnographic maritime groups.

Assuming that the data published in the literature (since Hallam's original construct was proposed in 1977) has failed to falsify these four proposals, we would be justified in pursuing Hallam's construct in relation to the initial peopling of the Americas. The following brief discussion examines the pertinent data in the literature.

The first proposal is not one which was explicitly inherent in Hallam's original construct as stated here. However, her mention of the human fossil-bearing sites of Niah Cave and Tabon Cave (Hallam 1977:130), and her discussion of early Australian skeletal specimens (Ibid:132–133), led her to speculate that the first settlers of Australia *may* have been a late representative of archaic *Homo sapiens* (Ibid:134). In this sense the first proposal presented here (above), represents a minor departure from Hallam's original statement.

The first proposal also alludes to the alternatives posed by the "out-of-Africa" and the multiregional models of human evolution—though from the standpoint of the current discussion the output of both models are roughly equivalent. What *is* significant in this proposal is that unless the presence of archaic *Homo sapiens* can be conclusively demonstrated for either Australia or the Americas, we must assume that peoples spread to these continents *after* the rise of modern *Homo sapiens* in East Asia.

Much of the discussion concerning the East Asian fossil record, its chronology, and its relationship to the multiregional and "out-of-Africa" models has already been presented at the beginning of this chapter. Uranium series date ranges of 140,000 to 119,000 and 125,000 to 100,000 B. P. for the Maba and the fragmentary Xujiayao specimens respectively (Chen and Yuan 1988:68) do not invalidate either model output. Similarly, the 67,000 B. P. uranium series date (Brooks and Wood 1990:288) for the fully-modern Liujiang skeleton fails to invalidate the predictions of either model.

This last date, however, will need further corroboration, since (to my knowledge) it represents the oldest chronometric date associated with fully-modern *Homo sapiens* in the Far East; while at the same time, it represents an especially young date for the Uranium series dating technique. Nonetheless, these concerns do not automatically invalidate this date, nor the projection that fully-modern *Homo sapiens* probably appeared in East Asia sometime between 50,000 to 70,000 years ago.

In this context it is significant that Chang (1986:68) places the transition from early *Homo sapiens* and the "Middle Palaeolithic" to *Homo sapiens sapiens* and the beginning of the "Upper Palaeolithic" of China at 50,000 years ago. Furthermore, Chang (1986:87) notes that archaeological assemblages dating to the "Upper Palaeolithic" of China become considerably more diversified and regionally distinctive than those of preceding periods.

Based on archaeological data, shortly after the appearance of fully-modern *Homo sapiens sapiens* in East Asia, and while archaeological traditions underwent significant changes in the "Upper Palaeolithic" of China, the first human settlers arrived in Australia and New Guinea. Although it is tempting to think that there is a causal relationship lurking somewhere between these events, I believe that it is still too early to speculate on such relationships.

In 1977 when Hallam published her construct, the earliest securely-dated sites from Australia reviewed in her article provided evidence for human presence back to about 35,000 years ago. Combining these data with data on sea levels, she concluded that the initial settlement of Australia probably dated to the preceding episode of low sea level dated between 50,000 to 60,000 years ago (Hallam 1977:134). This estimate, based on similar reasoning, was also tentatively accepted by Birdsell (1977:116). Based on an analysis of the skeletal evidence, Thorne (1977:196-197) suggested that the initial peopling of Australia may have taken place "... 60,000, 80,000 or even 120,000 years ago."

Although subsequent publications have tended to place the date of initial entry at 53,000 (or more generally at 50,000) years ago, most authors since the mid-1970s have tended to agree with Hallam's assessment of the date for initial entry into Sahul (cf. White and O'Connell 1982:31; Serjeantson et al., 1982:915; Flood 1983:74; Shutler 1983:43; Thiel 1987:238; and White and Lampert 1987:11-13). These assessments have been reinforced during the last fifteen years by the discovery of a number of additional early sites in Australia, some of which have dates approaching 40,000 years B.P. (Jones 1989); sites in Tasmania dating from around 30,000 years ago (Cosgrove 1989:1708; Jones 1989:767-770); and several Late Pleistocene sites in New Guinea, amongst which at least one site is dated at 40,000 years ago (Groube et al., 1986:453; Jones 1989:763-765).

Despite the fact that the initial populations arriving in Australia would have entered along the northern margins of the continent, many of Australia's earliest sites occur near the southern periphery, often along rivers and extinct Pleistocene lakes. A systematic analysis of this distributional pattern along with the character of the evidence presented by early sites led Bowdler (1977:205) to propose that

> ...Australia was colonised by people adapted to a coastal way of life; that initial colonising routes were around the coasts and thence up the major river systems; and that non-aquatic adaptations, such as desert and montane economies, came relatively late in the sequence. It is further argued that the early Australian colonists were not big game hunters, and that analogies drawn with the peopling of the New World are misleading.

Subsequent publications by several authors (White and O'Connell 1982:50-53; Flood 1983:78-79; and White and Lampert 1987:16-17) tend to suggest that Bowdler's construct may have been premature and difficult to test. Bowdler's construct contained several propositions beyond simply an emphasis upon a coastal route of entry. In a summary of Bowdler's construct, Flood (1983:78) states that

> Bowdler's thesis is basically that the occupation of the continent was by people adapted to living on the coast who gradually moved along the coasts around the periphery of the continent. It is argued that their marine-based technology and economy, with a heavy reliance on scale fish, shellfish,

> and small mammals, underwent little change for many thousands of years, but gradually they did expand inland along major river and lake systems. It was not until about 12,000 years ago, she suggests, that the dry heart of the continent was occupied, when people had developed techniques to hunt large kangaroos and began to use grindstones to exploit grass seeds as food.

I believe that we may separate the two primary components of Bowdler's construct and test them separately. The first component of the construct relates to initial entry and coastal spread of maritime-adapted peoples, followed by an inland spread along major river systems. The second component predicts that these adaptations continued in the absence of inland adaptations until the terminal Pleistocene. Separating these two components of Bowdler's construct allows us to evaluate each component independently.

It is clear from the literature that the second portion of Bowdler's construct has been falsified by the discovery of early sites such as Devil's Lair (White and O'Connell 1982:52), and a number of inland sites with dates in excess of 20,000 years ago, which clearly show adaptations to non-aquatic resources (Jones 1989:759). Indeed, as Jones (1989:757) points out, it is obvious that "...there was human presence in all major ecological zones [of Australia] by the time period 20-25 kyr ago." On the other hand, if one considers only those sites which have been securely dated beyond this time limit, it is equally clear that all such sites lie near coastal or inland riverine areas (cf. map in Jones 1989:757, and discussion of sites in this article).

Flood also voices one of the other common concerns surrounding Bowdler's construct:

> One of the basic problems in the search for traces of the earliest colonists is that the earliest sites will now be underneath the sea. The earliest campsites were drowned by the rising post-glacial seas, and not until the people had gradually moved a hundred or more kilometres inland, to what is now the coastline of northern Australia, would any traces of these first migrants remain (Flood 1983:79).

This last objection is quite reasonable, and unfortunately rather intractable. We must recognize, however, that our inability to get at the data can not be equated with the a priori assumption that such data do not exist. The overall geographic pattern of the earliest sites tends to support, not falsify, the first component of Bowdler's construct.

Finally, the discovery of a 40,000 year old coastal site on the tectonically uplifted coral terraces of the Huon Peninsula in Papua, New Guinea (Groube et al., 1986), provides direct archaeological support for the validity of early maritime adaptations in the Sahul; archaeological evidence of early occupation of Tasmania (Cosgrove 1989), Buka Island in the Solomon Island chain (Jones 1989:767), and Sulawesi (Jones 1989:753) all point to an early spread of maritime-oriented populations. Thus, we must conclude that the literature since has failed to falsify Hallam's (1977) construct, and indeed tends to support the first two numbered proposals made in this section.

I wish to stress that from the point of view of this discussion my acceptance of the first two proposals on the basis of the data reviewed thus far does not necessarily imply that there was a single, unified, biologically or culturally homogeneous spread of peoples with a maritime orientation around 50,000 years ago. Indeed, it seems likely that adaptation toward a maritime-oriented economy could have taken, and probably did take place at various different locations along the western Pacific.

What I am suggesting, however, is that some of the initial adaptations toward a maritime orientation may have been made prior to 50,000 years ago by mainland and insular East Asian populations of late archaic *Homo sapiens* or early *Homo sapiens sapiens* living in near-shore environments. Furthermore, it seems reasonable to assume, as Thiel (1987:238) has suggested, that the reduction in land area and coastline which took place as a result of a major rise in worldwide sea level around 50,000 years ago, would have provided the necessary impetus for coastally-adapted populations to search for new lands. This process would not have been limited only to the Sunda region, but probably affected all such populations along the shores of the western Pacific.

The importance of these arguments to an understanding of the initial peopling of the Americas becomes evident when viewed in the light of the early archaeological record of Japan. The last fifteen years have seen a remarkable set of developments in our understanding of early Japanese prehistory. Twenty years ago Morlan (1971:136) noted that

> There appears to be no unequivocal evidence of pre-Wisconsin maximum human occupation of Japan. Assemblages characterized by heavy tools of 'Lower Palaeolithic' types have been recovered but either the specimens are only questionably man-made or the stratigraphy and/or chronology are in doubt.

As a result of such assessments, Hallam's discussions of the early prehistory were necessarily vague. For example, she notes (Hallam 1977:141) that

> There have long been suggestions that both the elongate flake traditions of southern Japan (Kyushu, Shikoku, and southern Honshu) with a date range from more than 30,000 years ago (Fukui cave) to about 13,000 years ago; and the blade traditions of the north (northern Hunshu [sic] and Hokkaido from at least 17,000 BP) are superimposed on a 'heavy tool' technology (Morlan, 1971). Around 40 such 'early Palaeolithic' assemblages remain somewhat problematic, but geomorphologically some appear more than 130,000 years old, and others may be middle Pleistocene (Ikawa-Smith, 1975). The elongate flake (south) may develop indig-

> eneously [references in original].

In fact, during this period it seemed that early sites in Japan were clearly divided into two groups. Of the 1000 or so early sites known in Japan, the vast majority belonged to the Late Pleistocene (dating between 30,000 and 10,000 years ago), and contained highly variable tool kits. About 100 of these sites were claimed to be older than 30,000 years ago, and were classified as "Lower Palaeolithic" sites (Ikawa-Smith 1978a:42). Some of these "Lower Palaeolithic" industries were described by Aikens and Higuchi (1982:88) as follows:

> Several large handaxes or choppers from Gongenyama, of Paleolithic appearance, are undeniably of human manufacture. They resemble the classic Acheulian handaxes of Middle and later Pleistocene age from Europe and Africa, one of them strikingly so. They are believed to have come from a volcanic loam stratum laid down roughly 35,000 to 50,000 years ago, although this geological association is not securely established. Specimens said to be of similar type have also begun to turn up in small numbers at several sites in northern Honshu. At Myojinyama B and Kamiyaji B in Yamagata Prefecture, such artifacts have been assigned dates of 26,000 and 29,000 B.P., but their contexts and associations remain uncertain... Such an age ascription puts these sites in the same general time range as is estimated for Gongenyama, but if all these handaxe-like specimens are indeed of that age, then they are many tens of thousands of years younger than the Afro-European paleoliths they resemble. Assessment of their actual significance must await the accumulation of additional evidence, but the specimens are mentioned here because they represent a kind of lithic technology that might well have been present in Japan in early Paleolithic times.

Sites belonging to the "Lower Paleolithic" of Japan are also briefly reviewed by Ikawa-Smith (1982). In the region of the Kanto Plain north of Tokyo correlations with radiocarbon-dated tephra allow dating of these sites. On this basis, more than 50 sites whose data have been recovered under controlled conditions have been dated to beyond 40,000 years ago (Ikawa-Smith 1983:193-194). Ikawa-Smith (1982:21-23) notes that "recent work in this area indicates that irregular flakes with very little retouch together with heavy chopper-like tools continue to be used at least until about 20,000 years ago."

Datable volcanic materials are fairly common in many areas of Japan. Reynolds (1986:330) reports that fifteen sites have been located in the hills near Sendai in Miyagi Prefecture (on the northeast coast of Honshu) which occur under the Kawasaki Scoria dated by fission track to 30,000 years B.P.

> Workers in Sendai believe the sites to be as old as 50,000-70,000 years B.P. The lithic items recovered are a mixture of raw materials: andesite, quartzite, chert, and siliceous

> shale. Generally, the pieces are in fresh condition and many good flakes are present. There are also some tabular pieces and 'bashed lumps.'
>
> Also notable is the site of Zazaragi, Miyagi Prefecture, where in 1981 an 'Early Palaeolithic' site was discovered in excavations that had commenced in 1976. Fifty artifacts made on siliceous tuffs and shales were recovered from stratum 13. Among these pieces were points and scrapers. Some microwear study was undertaken on these pieces and suggests use on wood, bone/antler, hide/meat, and non-woody plants... This stratum has been dated by fission-track techniques to 42,600 ± 7,700 years B.P. and by thermoluminescence to 41,800 ± 2,800 B.P. A further layer below this, stratum 15, contained 15 artifacts, some exhibiting bipolar technique. This layer is dated by fission-track to 44,800 ± 7,700 years B.P. and by thermoluminescence to 40,600 ± 2,600 years B.P. [Reynolds 1986:330-331].

More recently two Japanese archaeologists, Koichi and Kazuto (1988), report on a series of recent discoveries with enormous potential significance to an understanding of the adaptations and probable maritime orientation of some of the pre-30,000 year old sites in Japan.

> The discovery of tools appropriate for chopping trees and splitting and shaping wood suggests that Paleolithic peoples in Japan lived a life closely adapted to forest conditions. Other evidence supports this. A piece of worked wood has been unearthed in peat deposits 32,000 years old at the recently surveyed Nogawa-Nakasukita site in Koganei, in the Tokyo metropolitan area; and a wood artifact thought to be a spear point, short sword, or oar, found at the Nishiyagi site in Hyogo Prefecture, is estimated to date from the early part of the last glacial period (70,000-60,000 B.P.). Paleolithic peoples in Japan apparently used a wide variety of wood as well as stone implements [Koichi and Kazuto 1988:246] [emphasis added].

The maritime orientation of the Late Pleistocene cultures of Japan is further stressed by Koichi and Kazuto. These authors point to recent exploratory borings in the Korean Strait which indicate that even during the full-glacial (21,000 to 18,000 years ago) Honshu remained separated from the Asian mainland. Furthermore, they point to the presence of early obsidian tools found at sites in the Kanto Plain. These tools were manufactured from material imported from the Izu Islands, which even at the height of full-glacial conditions were separated from Honshu by a body of water 200 meters deep and 40 kilometers wide [Koichi and Kazuto 1988:247].

In view of all the evidence which they had presented, Koichi and Kazuto conclude that

> The inhabitants of the region must have been capable of

> ocean crossings, that is, of making boats of some sort. This may be the key to a new way of thinking about the Paleolithic in Japan—as the culture of a forest people who worked wood [Ibid.].

Reynolds (1986) mentions still another site which he considers to be especially important; this is the site of Nakamine.

> Exceptional among these new data [from Japan] is the site of Nakamine, Miyagi Prefecture, excavated in 1983. At this site five cultural horizons occur, the lowest (5) is dated by thermoluminescence to 368,300 ± 6,200 years B.P. Lithic artifacts include amorphous flakes, some of which were produced by bipolar technique (Reynolds 1986:331).

Koichi and Kazuto (1988) help to round out further the picture of what is known about this early period in Japan. According to these authors,

> Concentrations of stone artifacts have been found in seven places around a swamplike depression in Bed 20 at Babadan A, just below a bed of Ichihazama Pumice laid down maybe 150,000, maybe 120,000, years ago, as determined by fission-track dating and thermoluminescence dating respectively. Fatty acids from animals—maybe elephant (Palaeoloxodon naumanni) and deer (Cervus nipon?)—have been detected on some of these artifacts, suggesting that hunters used them for butchering. Thermoremanent remagnetization indicates the presence of one spot with a greater magnetic intensity than any other, perhaps a hearth or bonfire site (Koichi and Kazuto 1988:245-246).

The nature of the archaeological materials from the oldest levels of Babadan A and the Nakamine C sites have collectively been summarized as follows:

> These stone tools are made of silicified shale, silicified tuff, chalcedony, jasper, and rough andesite and, with some differences in cultural sophistication, consist mainly of small flake tools (sidescrapers, points, and drills) and utilized flakes, in some cases accompanied by massive choppers and bifacially retouched tools. However, since no chips, the byproduct of stone-tool manufacture, have been found at any of these sites, there is no clear archaeological proof that the production of stone tools in fact occurred locally. They may, in other words, have been made elsewhere. These early paleoliths differ from late paleoliths in that they show little uniformity of shape, manufacture, or raw material (Koichi and Kazuto 1988:245).

Given such discoveries, it is not surprising that some Japanese archaeologists are now seriously considering the possibility that these islands were settled during the Middle Pleistocene (Akazawa 1989:1; see also Yoshizaki and Iwasaki 1986).

Obviously much still remains to be clarified about the "Early Palaeolithic" of Japan. These discoveries simply represent the beginnings of a new understanding of early Japanese prehistory; and changing perspectives—especially about very early sites such as the Nakamine and Babadan sites—may be accepted only slowly. For example,

> The date of first settlement [of Japan] has long been controversial. Homo erectus may have hunted in Japan, but the few stone tools claimed to document these archaic people are unconvincing. The earliest well-documented artifacts date to at least 50,000 years ago, mainly heavy choppers and crude flakes, and occupation levels in the 40,000-year range occur in the lowest levels of several Japanese caves (Fagan 1990:191).

Nevertheless, given the evidence reviewed here, it seems quite obvious that the Japanese islands supported human populations who probably had considerable maritime capacities by at least 50,000 to 70,000 years ago, and possibly a good deal earlier. Entry of human populations from East Asia to Japan may well have required an ability to cross varying amounts of open water, although this may not have been the case during Middle Pleistocene times. However, given the data from the sea cores from the Korean Strait indicating that Honshu was not connected to the Asiatic mainland by a direct land bridge even at the height of the Late Wisconsinan (Koichi and Kazuto 1988:247), and assuming that we accept the conservative view expressed by Fagan (above), which indicates that Japan was settled sometime just before 50,000 years ago, we must still conclude that the islands most likely were settled by seafaring voyagers from the mainland of Asia.

Given these data we must conclude that the third proposal (namely that the archaeological evidence from the northwestern Pacific littoral during the Mid-Wisconsinan further supports the existence of a maritime orientation northward [of Sunda and Sahul] along the Pacific rim) is clearly supported by the data reviewed in this section. Thus we are left with one final proposal to assess.

This last proposal involves comparisons of known ethnographic technoeconomic adaptations employed by insular and maritime peoples, to the Mid-Wisconsinan archaeological record of the western Pacific. For the purpose of the present discussions, we will narrow these comparisons to a consideration of some of the technological traits which characterize archaeological sites on the western Pacific dating around 50,000 years ago, and briefly compare them with some known ethnographic examples.

The richest source of data from the southwestern Pacific comes from portions of southeast Asia and adjoining insular regions which Peter Bellwood (1985) has called the Indo-Malaysian Archipelago. Unfortunately, problems in dating have plagued attempts at prehistoric reconstruction (cf. Bartstra 1978:33-34; Bellwood 1985:67).

Possibly the best current interpretation is offered by Bartstra and Basoeki (1989:243), who place the Pacitanian industries in this area between 60,000 and 10,000 years ago, and explicitly tie their appearance in the archaeological record with early Homo sapiens in Java. Preceded (in Java) by the Ngandongian industries from the Solo High terrace (Ibid.); the second half of the Pacitanian of Java overlaps chronologically with the the Hoabinhian (Late Wisconsinan/Holocene) of the Asiatic mainland and surrounding islands of the Indo-Malaysian Archipelago (Bellwood 1985:175).

The chronology and classification of early stone industries from Java presented by Bartstra and Basoeki (1989) designates the Pacitanian industries as Late Paleolithic expressions of the chopper/chopping-tool tradition. Additionally, Bartstra and Basoeki (1989:243) feel that

> ...the chopper/chopping-tool complex of Southeast Asia is considerably younger than is usually thought, most of the large core industries there belonging to the second half of the Upper Pleistocene and in some cases even to the Lower Holocene.

Their use of the term "chopper/chopping-tool complex" (above) in this context is but one expression of the general difficulties which surround chronological and classificatory issues in this region. Indeed, some authors have explicitly rejected this terminology (e.g., Yi and Clark 1983), while others continue to use the term if only as a convention or a convenient point of reference for subsequent discussion (cf. Bellwood 1985:56).

> My own observations of these classificatory problems stem from my research on later Pleistocene and early Holocene industries, including the Hoabinhian of Malaya and contemporary industries in Borneo and Sulawesi. There seems little doubt that these are the direct but regionally diversified descendants of presumed Pleistocene ancestors such as the Pacitanian and its Mainland South-East Asian allies. I have elsewhere referred to all of these industries as 'pebble and flake industries'; in some remote parts of South-East Asia they have continued in production into recent historical times, although new technologies did appear in certain regions during the Late Pleistocene and Holocene (Ibid.).

Thus, although the two should not be directly equated, in a way, the new descriptive term "pebble and flake industries" attempts to overcome the less useful connotations of the "chopper/chopping-tool industries" of an earlier generation.

In what amounts to a concise, modern summary of these "pebble and flake industries," Bellwood (1985:57-58) divides the lithic components into three principal categories (heavy tools, smaller tools, and manufacturing waste), describing them as follows:

> a. Fairly heavy tools made by flaking the edges of riverine pebbles, large flakes or quarried nuclei—these are normally called 'pebble tools' in popular parlance and include most of Movius' categories. Variables such as edge length, edge

> position, edge angle, and extent and position of surface flaking and unmodified cortex all intergrade. The distinction between unifacial and bifacial working can often be used with profit, especially to isolate an interesting category of bifacial 'hand-axes' in the Javenese Pacitanian... Another useful distinction can perhaps be made with edge angles; the pebble tool forms which Movius defined as choppers, chopping-tools and hand-axes tend to have fairly low edge angles, but his hand-adzes and scrapers seem to belong to a differentiable category of steep-edged, flat-based and thick unifacial tools, usually with edge angles of over 70 ...
> b. Smaller *tools* made on true flakes struck from cores... or sometimes on small chunks of stone. These rarely have specific edge or shape characteristics, and if they do not reveal clear patterns of retouch they can only be separated from purely waste material by edge inspection.
> c. Manufacturing *waste*—items not in categories a. and b. because they show no signs of post-manufacture retouch or usage. They include unutilized flakes, chips [that is flake-like pieces lacking bulbs of percussion, caused either by core smashing or flake breakage], shattered cores and chunks, and sometimes true discarded cores. The latter are often hard to recognise as true debitage since the removal of flakes causes edge-shattering which can easily be confused with use-damage. The term 'horsehoof core'...is used in Australian archaeology to describe one very characteristic single-platform variant of this class, but there is often confusion as to whether these are to be considered as steep-edged tools or waste items—each example must be examined individually. Other core shapes are less distinctive, but spherical and conical trends in shape can often be observed [emphasis in original].

Both Ikawa-Smith (1978a:45-51) and Aikens and Higuchi (1982:35-94) provide synoptic summaries (some with illustrations) of Palaeolithic sites from Japan. Several of these sites (e.g., Iwajuku I, and Gongenyama) dated near 50,000 years ago contain assemblages which are very similar to the "pebble and flake industries" or Pacitanian industries described by Bellwood (above). Indeed, speaking of the Gongenyama site, Aikens and Higuchi (1982:42) note that "the first two assemblages have been compared with the Patjitanian [i.e., Pacitanian] Paleolithic industry of Java, which is said to contain some handaxe-like artifacts as well as simple choppers and flake tools."

Interesting, also, is the interpretation given to these industries—both in the southwestern Pacific region and in Japan. Describing the Paleolithic assemblages of East and South Asia, Ikawa-Smith (1978b:8) says

> ...what characterizes this vast area and temporal span are the flakes, which are often amorphous and which were utilized with very little modification. Choppers, chopping

> tools, proto-handaxes, and even true handaxes occur with flakes in varying frequencies. Undoubtedly we are looking at the skeleton of the prehistoric people's tool kit. The flake tools may have served for cutting and scraping meat and vegetable materials, and heavy tools for chopping and smashing, but the daily tasks of obtaining foodstuff, preparing meals, keeping the shelter in order, and carrying out rituals surely would have required other tools and equipment made from bamboo, wood, bone, antler, shell, and other organic materials. The stone tools, in general, probably served to make these other artifacts.

More specifically, when we examine the interpretations of the archaeological record which have been posited for Southeast Asia,

> Several of the archaeologists working there have hypothesized that the lithic toolkit known for the region was primarily used to make wooden tools and that this was the reason the tools were simple, little-changing, and primarily unifacial, distinct from those of the 'west'... What little edge-damage research has been done tends to support this tool function (Solheim 1983:197).

The similarities in tool kits between Japanese sites dating around 50,000 years ago and Southeast Asian "pebble and flake industries," together with similar interpretations emphasizing generalized stone tools used in working wood and bone (see Koichi and Kazuto 1988, plus earlier discussion) offer significant support for the proposition that technoeconomic adaptive patterns were potentially very similar along the western Pacific littoral. Unfortunately, with the remarkable exception of some rare wooden tools from Japan (see above), most of the material culture must be inferred from these lithic remains.

Perhaps Bellwood (1985:68) is correct when in reference to the southwestern Pacific sequence he states:

> ...I am quite happy to agree that the tools appear to be simple, amorphous and undifferentiated to a modern observer with a fairly Eurocentric (or Afrocentric) bias. However, they do have durable and usable edges and I see no reason why they should not have served their intended purposes with a quite acceptable level of efficiency... What may be more important is that there is no evidence for intensive regional cultural specialisation in stone tool manufacture in South-East Asia, even during the Late Pleistocene. Here there may be good reason to welcome the observation of Service (1970) that being unestablished (that is unspecialized) is an evolutionary privilege from which many historical societies have greatly benefited in the long term [reference in original].

Unfortunately, for the purposes of comparison with historically or ethnographically known technologies, the logic of Bellwood's reasoning does not yield any tangible solutions. Although indirect evidence suggests that groups using a "pebble and flake industry" must have had a maritime orientation, this is not the same as finding direct evidence in the form of obviously specialized maritime-oriented artifacts. Still, Bellwood's statement does raise another issue—one of technological competence.

The issue of technological competence in relation to a maritime orientation is difficult to address directly. Most of the early artifact assemblages (discussed above) appear to constitute portions of a generalized tool kit containing elements appropriate to working wood or bone (i.e., tools to make or maintain other tools). Thus, specialization (or lack of specialization) of the portions of the total material culture inventory made from perishable materials remains can not be directly tested.

Another approach which could be used is to compare these archaeological specimens to the tool kits of historic or ethnographic maritime groups. One potential starting point might be the historic/ethnographic comparative work of McCartney (1975), who compared the maritime adaptations of the Aleut of the north Pacific, to the Yahgan and Alacaluf of Tierra del Fuego and southern Chile. In this work McCartney summarizes in table form a comprehensive set of comparisons (including environmental, technological, settlement traits) for these groups (1975:318-322). Based on these comparisons, McCartney concludes that "Fuegian artifacts for the most part strongly resemble Aleutian artifacts" (1975:326).

Clearly there are a number of differences in raw material, style, decoration, and relative emphasis on resource utilization. Nonetheless, McCartney's focus was centered on comparative adaptive patterns, and not on any attempt to demonstrate closer relationships between these groups. In this sense, his comparisons are both vivid and convincing.

Even more interesting from our perspective is his comparative list of 28 types of implements (the 29th was "no pottery") used in the two areas during pre-contact/early contact times (McCartney 1975:320-321). Out of these 28 entries, 19 (or 68 per cent) referred to material culture items made from perishable materials such as wood, bone, skin, and seaweed. Eight (29 percent of the remaining entries) referred to items made from stone; while one (3 per cent) occurred as specimens made from either stone or bone. These last nine items would be the only specimens which might be directly compared to archaeological assemblages lacking artifacts made from perishable materials.

McCartney's list of non-perishable implements includes:

11) Chipped, ground slate and bone projectile points
12) Chipped stone knife blades and scrapers
13) Polished stone ax/adze blades
14) Long, fine-textured whetstones
15) Pumice abraders
16) Rounded hammerstones
17) Scraper blades of chipped stone
18) Stone sinkers
19) Bolas (?) rare (McCartney 1975:321)

Obviously only some of the projectile points (item 11) are nonperishable, while slate as a raw material or grinding as a means of producing lithic projectile points are traits which are dependent on available raw materials and cultural traditions rather than on a maritime adaptation. Furthermore, some of these projectile points were used to tip arrows, which represent a technology which has not been demonstrated to have been present in the Mid-Wisconsinan western Pacific cultures. Still, some form of stone projectile points remains a possibility.

The remaining eight classes of implements can be broadly divided from an archaeological perspective on the basis of their relative "visibility." By this term I mean that some artifacts will show human modification which results in combinations of shapes and workmanship types which are clearly the result of human activity, while others are considerably less obviously the result of intentional shaping. The first step in this set of contrasts involves the difference between implements which have been identified as man-made products historically or ethnographically, as opposed to archaeologically recovered items which are interpreted to be the result of human modification.

Thus, "low visibility" items such as whetstones, abraders, rounded hammerstones, stone sinkers and bolas—particularly if roughly made or not produced to conform by size or shape to a recognizable pattern—are likely to be overlooked, misinterpreted, or dismissed altogether. They may or may not be identified in archaeological contexts; their significance may not be apparent; they may be classified as problematical or unimportant; they may go unreported; or they may be dismissed as natural objects.

By contrast, "high visibility" items in McCartney's list would include stone ax/adze blades (either flaked or polished), and flaked stone items such as knives and scrapers. These are in fact precisely the prime components of the archaeological "pebble and flake industries" outlined by Bellwood, above. Such contrasts are further strengthened (particularly in the case of "low visibility" implements) by the realization that many of these items need not be made to exacting standards of style or efficiency; nor need they be intended for repeated use, transport, and regular maintenance.

A number of authors have provided indirect confirmation of this conclusion relative to the Fuegian groups. One of these authors (Cooper 1917, 1946) has provided overviews of the subsistence and technology of the historic Yahgan. Of paramount importance to the Yahgan diet were shellfish, followed by fish, sea mammals, birds and eggs; while berries, fungi, and rarely fox, otter, and guanaco completed the diet (Cooper 1946:84).

Much of the food of the Yahgan was obtained directly by hand. Since they lacked fish nets or hooks, even fish were commonly lured to the surface with bait and grabbed by hand (Ibid.). Birds were captured with a simple pole snare; seals and cormorants were killed with clubs; and crabs and sea urchins were gathered with simple three or four-pronged sticks (Ibid.).

In addition to these implements the Yahgans used a simple bone harpoon or spear, a hafted mussel-shell implement which functioned both as a scraper and as a knife, wedges made of bone, sinew and whale gut lines, seal-hide thongs, wood and bone awls, and a variety of basketry types (Cooper 1946:89-91). "The bow and arrow, in contrast to Ona usage, occupied a very subordinate position" (Cooper 1946:91). "Little use of worked stone was made except for arrowheads and, rarely, scrapers. The workmanship was crude" (Cooper 1946:89).

Steward and Faron (1959:400) note that "the inventory of tools, weapons, and other gear was limited to the barest necessities, owing in part to difficulties of transporting them in small canoes." Thus it seems that with frequent movements and limited transport capacities, it made sense to produce objects for immediate use—in other words, "...the Fuegians developed a kind of 'throw-away' culture involving few permanent implements to be transported in their wanderings" (McCartney 1975:322).

One set of contrasting terms used by Binford (1973, 1977, and 1979) describes the differences between expedient tools (of basic functionality intended to satisfy only the needs at hand); as opposed to curated tools (representing greater energy investments in manufacture and repeated maintenance and transport in anticipation of repeated or future use). In the context of the present discussion, it might be said that the Yahgan made and used few curated tools; and very few of these were made from stone.

The historic/ethnographic record of the Yahgan clearly demonstrates the viability of their technoeconomic adaptations to a maritime existence near 55 degrees latitude south. The archaeological record (cf. Bird 1938), on the other hand, is not nearly so conclusive. It is doubtful that anyone would reach this same conclusion based strictly on the study of their archaeological remains, especially after the passage of several tens of thousands of years.

In Ikawa-Smith's (1978b) words we would be looking at the mere "skeleton" of their tool kit, since all the perishable components would be missing. Furthermore, we would miss, ignore, or dismiss most of their "expedient" technology which had survived; and in the end we would be left with their few "curated" and some of the more recognizable "expedient" tools made from stone; while we were left to ponder their original significance.

Given the similarities between the archaeologically recoverable and "visible" elements of the technology of high latitude maritime-oriented groups like the Aleuts and the Yahgan, and the descriptions of Mid-Wisconsinan archaeological assemblages from the western Pacific littoral, we must conclude that we have failed to falsify the fourth and last proposal with which we began this section. Furthermore, in light of the data and reasoning presented in this section, we must conclude that the portions of Hallam's original (1977) construct discussed in this section have all failed to be falsified by the literature since 1977. Hallam's original construct is reinforced, while the likelihood remains strong that the Americas were initially peopled in the Mid-Wisconsinan by peoples spreading along the littoral margins of the Pacific.

Our review of the related paleontological, archaeological, and ethnographic data provided specific and detailed support for each of these proposals. As with any other working construct, we must acknowledge that our conclusions are transient in so far as we are limited by the available data. Nevertheless, our review failed to falsify any of the four original proposals.

The data reviewed in this section does not constitute compelling evidence, but it does provide a strong support for the proposition that,

a] an early spread of coastally adapted, anatomically modern peoples took place along the western Pacific rim around 50,000 years ago; that

b] these early populations moved along the East Asian coasts, both northward and southward, and reaching islands presumably with watercraft; that

c] these early groups adapted to coastal habitats using a fairly unelaborated lithic tool kit, together with other, more complex perishable items; and that

d] comparably adapted ethnographic groups using similar technologies are known to have successfully occupied high latitude coastal environments in the past.

Although the proposition that the Americas were peopled near this time by the spread of such coastally adapted groups must be demonstrated using archaeological data from the Americas; the review of the data which support the four proposals presented in this section provide a reasonable and defensible context within which related archaeological data from the Americas can be integrated.

Chapter Five

EARLY NATIVE AMERICAN PREHISTORY: THE MODEL OUTPUT

This chapter presents a model output of early Native American prehistory which incorporates the full breadth of data and conclusions presented in the two preceding chapters. Although not cited, these data are drawn from a variety of sources; however, the early archaeological record of the New World has been intentionally excluded. As it was pointed out as part of the discussion presented in the second chapter, only by this exclusion can we hope to provide independent means of evaluating the validity of our archaeologically derived constructs. Reference will be made, as appropriate, to earlier sections from which the proposals presented in this section have been drawn.

The model output contains a variety of specific theoretical projections of past events, some of which will be directly testable at present, while others will require the accumulation of additional data. The intent is to produce a set of projections which are complete and consistent, both internally, and with the data presented thus far. Once stated, these projections will become the base line from which hypotheses can be derived for direct testing. The comprehensive nature of this model necessarily implies that some of its projections will exceed our current archaeological data base, or our capacity to devise critical tests appropriate to our available data base. This feature, however, should not be viewed as a weakness, but rather as a strength of the model—opening avenues for future research and testing.

The wide-ranging and complex data and principles presented in earlier sections of this work will be incorporated into a sequence of six processes arranged in rough chronological order. Their labels convey to some extent the sequence of principal events which the model output projects for the Mid to Late Wisconsinan of the New World. Although these processes might almost be viewed as chronologically sequential phases, I have resisted presenting them in this light, since local, regional, and stochastic factors may render such a strict linear arrangement unlikely. In other words, it is likely that some of these processes (particularly the third and fourth process) may have occurred locally more than once in synchronous, or even in repeated cycles.

Keeping in mind these potential problems, the following represents the sequential list of the processes proposed by the model output: 1) Entry (initial entry into the Americas); 2) Dispersal (initial spread of peoples); 3) Diversification/Adaptation (adoption of appropriate strategies on a local basis); 4) Saturation (gradual population growth and expansion to include less desirable resources and habitats until equilibrium levels are reached); 5) Crisis (cultural responses to the climatic and biotic shifts in the period preceding the end of the Pleistocene); and 6) Specialization (the rise of successful adaptations to terminal Pleistocene/early Holocene conditions).

5.1 ENTRY

Although portions of southern Siberia, northern China, and Japan may well contain Middle Pleistocene sites, the extension of these early peoples into the higher latitudes (especially in continental areas) necessary to achieve entry into the New World remains undemonstrated at present. The current lack of fossil specimens other than those of Homo sapiens sapiens in the New World also suggests a later date of entry.

The paucity of evidence for human presence in high latitude areas of Asia may well reflect the action of a combination of factors. The human organism developed in, and retains biological traits which are most appropriate to the tropics. Although cultural adaptations allow us to overcome environmental barriers (or gradients of increased difficulty) and to function successfully in non-tropical areas, there is generally a direct relationship between the energy expended to overcome environmental barriers and the number or severity of these barriers. Furthermore, energy expended in small-scale societies on cultural adaptations must both be balanced against human physiological and reproductive needs, and it must be compensated by an increased intake of food energy (section 3.1).

High temperate to arctic latitude environments have decreased solar energy receipts and decreased productivity. Often decreased precipitation effectively turns many such areas into deserts. Seasonal differences in solar energy receipts become more extreme toward the poles. Growing seasons of plants are clearly limited to portions of the year; and most plant energies are invested into the production of cellulose (which is not digestible by humans), and into rapid reproduction. These effects are especially pronounced in the continental interiors, away from the moderating influence of the oceans. Ambient temperatures are low, particularly over seasonal periods of decreased sunlight.

These characteristics of high latitude continental environments constitute major environmental and physiological barriers to humans. Small-scale societies can and did eventually overcome these barriers in prehistory, but only by developing cultural systems which simultaneously addressed all the pertinent problems posed by these environments. Technology, economy, social structure, in fact all aspects of daily life, had to be re-shaped in an effective integrated systemic combination in order to adapt successfully to these environments. This development was necessary since failure to resolve any one of these problems would have rendered continued existence in these environments impossible. These interrelationships between cultural and environmental factors describe well the conditions characteristic of "restrictive environments" (see section 3.1).

Any environment which imposed fewer restrictions by presenting fewer or lessened obstacles to small-scale societies (i.e., a more "permissive environment") would have been occupied sooner than more restrictive environments. It is therefore significant to contrast high latitude continental (restrictive) environments with maritime /coastal (more permissive) environments which would have relaxed many (though obviously not all) of the barriers discussed earlier (for a detailed discussion of marine environments occupied by modern groups see Yesner 1980, with comments by other authors therein).

The data from physical anthropology, genetics, and demography (reviewed in sections 4.3.2 through 4.3.10) strongly suggest that initial entry of peoples (who have left modern descendants) took place during the Mid-Wisconsinan. The most likely time of entry was probably around 40,000 to 50,000 years ago. This time range is incompatible with our current understanding of the age of far Northeast Asian Upper Paleolithic cultures such as the Dyuktai (see section 4.5), but it matches well the timing of the early development of maritime adaptations along the western Pacific in general (section 4.5.1). Based on these observations, the model output proposes initial entry into the Americas by a coastally-adapted people spreading along the northern Pacific margins around 40,000 to 50,000 years ago.

Given the projected nature of early or unelaborated maritime adaptations (cf. section 4.5.1), it is difficult to visualize initial entry into the Americas as a continuous flow or a sudden flood of peoples. Coastal groups may employ watercraft both for subsistence activities and for short-distance travel. Subsistence along coastal areas is dependent upon a group's ability to access a wide variety of resources which require regular logistic movements within the group's territory. On the other hand, the roughly linear distribution of key resources reduces a group's capacity to survive repeated encroachment by neighboring groups, while heightening the need to defend one's territory.

The linearity of territories and the resultant linear distribution of human populations might also pose serious problems to human mating networks (cf. Wobst 1976). Mating networks composed of groups whose total numbers never exceed 300 to 400 people due to resource limitations may ultimately lack long-term viability (cf. Keegan and Diamond 1987:64). Thus, successful coastal adaptations must balance a tendency to disperse to insure adequate food supplies despite resource fluctuation and encroachment by neighboring groups, while maintaining mating networks of viable size whose members are sufficiently close to each other for continuing contact, given local conditions and travel abilities.

Although coastal resources may have been available throughout much of the north Pacific rim during the Mid-Wisconsinan, it is doubtful that natural conditions necessary to meet both opposing requirements were spread in a continuous line along the coast. Some areas may have been unsuitable for human settlement, some may have served as temporary stepping stones for largely transient small populations, while still other areas were capable of long-term maintenance of human populations of viable size.

This situation would have resulted in a broken or discontinuous distribution of settled communities with different degrees of long-term viability, separated by significant distances where local conditions did not favor permanent settlements. Over long periods of time these various communities would have increased or decreased in number, some of them ultimately disappearing altogether. Only an area's capacity to support sustained growth up to or beyond about 500 individuals would have insured long-term continuity for the groups composing that particular mating network [cf. Wobst 1976]. In this context we may note that archipelagoes are particularly important in this respect because islands tend to concentrate large quantities of coastline within a small area—thus increasing the likelihood that a viable population size might be reached.

Given the discontinuous distribution of territories favorable to long-term viability of human populations, and given both territoriality and limitations on successful long-distance travel, the establishment of a sequence of settlements along coastal areas must have proved an effective barrier preventing the lateral spread of new encroaching populations. Only the end population in such a sequence, facing virgin territory and having no neighboring groups on one side, would have had a reasonable chance to produce successful splinter groups.

Based on genetic data (see sections leading to 4.3.10), it appears that somewhere in this sequence of coastal population spread through splintering, minimally one such event was characterized by the creation of an effective splinter population of smaller than 800, and most probably around 100 individuals who thereafter became effectively isolated from their parental populations. The descendants of such a hypothetical end splinter group, in all probability, went on to become the first Amerindians.

At present there are no direct and compelling paleogeographic clues which might provide additional clarification of the actual mechanisms which led to the isolation of an end splinter group. However, this hypothesis, based on the dynamics of sampling error, is supported by the earlier discussions on blood groups presented in chapter four. It can even be argued on genetic grounds that if the descendants of a hypothetical end splinter group increased in numbers disproportionately to the remaining coastal populations, the numerically superior descendant populations south of the ice would have formed a new genetic "center" [cf. Wolpoff et al. 1984]. The existence of a new genetic "center" would have dominated regional gene flow patterns, while relegating the less numerous and more dispersed coastal populations to a peripheral role along the "edge" of a network of populations.

5.2 DISPERSAL

Demographically, the processes of dispersal, diversification/adaptation, and saturation can be viewed only in a very general sense as equivalent portions of a population growth curve known as a "sigmoid growth curve" [cf. Odum 1971:184 and Figure 3.2]. Two important considerations distinguish these portions of the model output from a simple sigmoid growth curve.

First, a sigmoid growth curve in ecology describes the demographic behavior of a _single_ population within a _single_ ecosystem. By contrast, dispersal, diversification/adaptation, and saturation in the model output describe the collective demographic behavior of many populations in numerous separate ecosystems. As such, these components of the model output represent grouped data which ignore individual variability and potentially long periods of stasis between dispersal events.

Second, the equation of the processes predicted by the model output to a sigmoid curve does not address the basis by which the time element (within which these demographic behaviors took place) can be described. Obviously, the rate of demographic change will significantly influence the shape of the demographic curve which is produced. This difference can be visualized by comparing the sigmoid curve in Figure 3.2 with the hypothetical population growth curve shown in Figure 4.1. The growth curve depicted in Figure 4.1 contains at least one sigmoid curve (most visible around the fourteen thousand year mark), however its shape lacks the vertical exaggeration shown in Figure 3.2. Thus the sigmoid curve represents a convenient aid to visualization, and a point of departure for further discussion.

The dispersal of populations in mid to low latitude environments must have presented each splinter group with a succession of gradually more diversified ecosystems. This trend should not be viewed as an absolute rule. The richness of ecotonal coastal environments would have been characterized by a gradual rise (toward lower latitudes) of increasing numbers of favorable terrestrial microhabitats and terrestrial resource types (i.e., more "permissive" habitats). These habitat changes would have been reflected by the increased importance of plants and small terrestrial animals in the diet of generalized foragers.

Fresh water sources and riparian habitats along permanent waterways mark the greatest concentration of such terrestrial resource types. A generalized subsistence strategy necessary for accessing the varied resources encountered along coastal environments would have also been well suited to accessing diversified terrestrial riparian resources. The general ecological similarities between coastal, and riverine and lacustrine near-shore environments suggests that the easiest and therefore probably the earliest shift inland would have taken place by way of various rivers with external drainages to the Pacific.

The model output predicts that the spread of populations (including the initial spread through mid to lower latitudes) took place unevenly over the total landscape. As stated earlier, riverine routes would have been likely avenues of early population dispersal. The sequence of habitats occupied by foraging groups reflected local ecological and habitat conditions relative to standing subsistence strategies and energy returns, rather than absolute geographical area or distance. Rich habitats presenting favorable conditions for generalized foragers (i.e., "permissive" environments) would have promoted human population growth and increased production of emigrating splinter groups differentially when compared to less favorable habitats. It is only as an outcome of these predictions that the model proposes an initial spread of populations along maritime and riparian environments. Other permissive environments, where available, may also have seen early occupation.

The preferential spread of early populations along ecologically favorable zones reflects the assumption that early populations in the Americas relied heavily on individual foraging through the use of a relatively generalized technology. This assumption supports general predictions about the non-random, preferential spread of early populations. Although the details of local environments during specific times in the past must be reconstructed from paleoenvironmental data before one can evaluate the potential suitability of given areas in the past, some generalized predictions are possible. Equable environments with limited extremes in temperature and precipitation would have presented fewer hardships to overcome. Ecologically diverse and productive environments would provide a greater abundance of food (prey encounters) of different types. These types of environments are well suited to generalized (broad niche) foraging adaptations. On the other hand, monocultural environments (such as modern grasslands), xeric, or very cold environments require special efforts, or the use of specialized skills and technologies which are typically met through systemic forms of adaptations (Butzer 1991:146-148 uses a similar line of reasoning to explain archaeologically observed population distribution patterns in the Paleolithic of southern Africa).

One of the advantages of maritime adaptations cited by Yesner (1980:730) is lowered dependency ratios. In other words, in maritime environments many resources are directly accessible by the young and the old, so that adult males and females in their prime are significantly relieved of the need to support these groups. Relative to earlier discussions (see section 3.1), this situation represents a subsistence pattern characterized by "individual foraging" strategies which are most successful in "permissive" environments. Ecologically this strategy represents a broad niche adaptive pattern in which people occupied a generalized omnivorous forager niche. Similar projections can also be made about the advantages of other ecotonal environments such as riverine and lacustrine shoreline environments.

The transfer of an individual foraging pattern from maritime resources in one local setting to a new (though similar) local setting, was most dependent upon gaining intimate familiarity with the local habitat, and developing effective strategies for sustainable long-term access of local resources. Splinter groups entering a new local habitat would have initially had to cope with several serious problems. First, lacking an intimate knowledge of the local habitat, they would have enjoyed a lower foraging success ratio to expended energy, compared to the parental group or compared to subsequent generations in the same habitat. The location of food and other resources within a new environment would have to be learned anew.

Second, it would be a mistake to assume that the niche occupied by splinter groups in new habitats were initially "empty" of non-human competitors. Healthy and stable ecosystems require the presence of both predators and prey species. Human foragers would have come into immediate competition with non-human predators for existing prey species in new local habitats. Successful colonization of new local habitats would have required the competitive displacement (cf. Cox et al. 1976:64) of existing non-human foragers or predators. Generalized foragers exhibit the advantages associated with feeding flexibility; as competition reduces the availability of any given food source, specialized feeders will outcompete generalized feeders for any given food source. Therefore, a broad niche generalized forager (in this case, human foragers) may not easily displace more specialized feeders.

Third, new localized habitats would have presented splinter populations with a new set of unknowns, and potential as well as real dangers. The local "mix" of resources and prey is never the same from one locale to the next. These differences could create temporary acute shortages—though such shortages would be less severe among primarily "individual foraging" groups than among systemic foragers. The physical habitat may have posed additional new dangers which familiarity and shared knowledge and experience could ultimately overcome. The local "mix" of the ecosystem may also have included real danger from large predators.

Finally, individuals of mating age would have had to have continued access to mates. In other words, a move to a new local habitat resulting in greater isolation and distance from other groups would have (perhaps at times drastically) reduced the number of available mates in the mating network. Serious reductions in the availability of mates of the right age and sex would have also limited the reproductive (and therefore the overall) success of splinter groups. The impact of all these factors in varying quantities could and probably did significantly influence the success of splinter groups in colonizing new local habitats.

It is probably important to point out that whereas exploration by individuals and even attempts at emigration by splinter groups was not necessarily confined to optimal new habitats, splinter groups settling in hostile or suboptimal habitats disappeared or languished, while splinter groups encountering optimal conditions thrived (see additional discussions of failed colonization hypotheses offered by Beaton 1991, and Butzer 1991). Any advantage realized by a new splinter group could be invested in overcoming other problems. These adaptive dynamics argue strongly for the maintenance of traditional modes of subsistence during the initial phases of colonization, favoring maintenance of individual foraging strategies, and a maritime or riparian orientation wherever possible.

Beyond these predictions it is nearly impossible to predict the exact direction and speed of the spread of these initial populations. Cultural and environmental variability add a large number of stochastic variables which makes any more detailed predictions about migration rates and routes relatively meaningless. The most significant predictions, then, are that the initial spread of populations would have taken place preferentially along coastal and riverine/lacustrine margins first (rather than uniformly across various habitats and ecosystems); that the likelihood of populations spreading inland, along waterways, increased as populations moved toward lower latitudes; and that the rate of population spread would have been very slow. This last conclusion is supported by the importance of familiarity with the local habitat to the success of individual foraging modes (see section 3.4). This pattern results in strong sessile tendencies while precluding rapid population dispersal rates.

When the preferentially uneven and slow spread of these early populations is coupled with relatively slow demographic growth rates, one must conclude that: 1) throughout the dispersal period (and even at the end of this period) human population numbers in the Americas remained vanishingly low; 2) very large segments of the Americas remained uninhabited, while the inhabited areas were arranged in a discontinuous patchwork of small populations focused on waterways and maritime zones; and 3) the continued existence of any one individual local group was never assured—some groups growing, others remaining stable, while still others disappearing within a few years or a few generations.

5.3 DIVERSIFICATION/ADAPTATION

At the beginning of this chapter I pointed out that it might be unwise to interpret the sequence of the projected processes listed in this model output as having occurred universally in a single chronologically linear sequence. Entry, diversification/ adaptation, and saturation could all be viewed from a myriad of local perspectives whose timing and sequence may not match other local, regional, or continent-wide sequences. Thus, once again, the projections of this model should be viewed as referring to a broad scale sequence of events which are modeled after more localized patterns.

The primary events which characterized diversification/ adaptation include on-going culture change, isolation from parental populations, local development of appropriate technoeconomic strategies, and a continued slow rise in total populations, resulting in the eventual significant initial occupation of non-maritime/ non-riparian habitats.

Throughout the diversification/ adaptation period, most extant New World populations continued to live along coastal areas and along major waterways, although in a number of highly-productive areas populations became more dispersed, significantly increasing the proportion of non-maritime/non-riparian areas inhabited in the Americas. Tropical and subtropical regions, where homeothermy energy costs were less demanding, probably saw this trend earlier than more temperate zones. An important factor favoring maritime and riparian environments in cooler latitudes is the availability of shellfish, which are easily obtained, and high in calories.* In tropical regions one indirect limiting factor may have been increased precipitation. Dense tropical jungles today are rich in plant biomass, but contain significantly fewer animals than are found in less densely wooded environments. Although the composition of plant and animal communities of Pleistocene-age would have been different, the ecologic mechanisms which account for the contrasts observed in modern ecosystems was probably the same as today. Local populations responding to the combined effects of variations in isolation and local habitat conditions (along with culture change brought about due to continued group fissioning) are very likely to have produced a great diversity of cultures (see section 3.5), while remaining conservatively based on variant modes of individual foraging strategies.

Climatic and biotic latitudinal zonation was not totally absent, but was much less sharply defined in the Wisconsinan than in the Holocene (see section 4.4.2). The resultant fine-grained environments represented a rich and diversified set of ecosystems which were ideally suited to the needs of generalized foragers (cf. Guthrie 1984:267).

The broad niche adaptation of groups primarily reliant upon individual foraging strategies meant that they were well equipped to exploit the diversified ecosystems which characterized many Pleistocene environments.

Dispersing populations still had to collect new territory-specific information and adjust to each local habitat. Furthermore, persistent differences in resource concentrations at and away from shorelines and waterways constituted a major barrier to rapid settlement of inland areas. Locating reliable sources of drinking water must have gained new importance once groups moved away from near-shore or riparian environments. These problems were probably less severe or even minor in wet zones. Furthermore, the drop in overall productivity per unit area would have resulted in a greater dispersal of populations—further decreasing population densities. Decreased population densities, together with a need for maintaining a viably sized mating network, might have limited the ability of populations to move into and occupy new environments permanently.

Given these limitations, the resultant pattern of dispersal would have added to existing coastal and riparian settlements various inland areas characterized by lower population densities and often occupied only ephemerally. Subsistence strategies would have continued to emphasize individual foraging focused on wide and diverse prey types (mostly plants and smaller animals), although some limited forms of local specialization and the some incipient new systemic foraging modes of local significance may have begun during this period.

5.4 SATURATION

Systemic foraging in the overall subsistence patterns of the preceding periods remained relatively unimportant. This pattern continued unchanged during the saturation period. The term "saturation," in this context, refers to the attainment of a theoretical upper limit of the collective population numbers of human foragers in the Americas relying mainly on individual foraging modes of subsistence. This does not represent a specific number, since it represents the result of a dynamic equilibrium (the upper or asymptote value of a sigmoid curve - see section 3.5) which is dependent on total area, specific environmental parameters, and the specific subsistence and demographic factors influencing a vast mosaic of local populations.

Systemic foraging strategies are forms of foraging specialization which are best suited to the needs of populations which can not sustain themselves primarily through individual foraging. The differences between the two types of strategies can best be understood by considering a hypothetical situation. In any given environment initially devoid of human foragers yet capable of sustaining human populations through either individual foraging or systemic foraging strategies, each alternative will present different problems and solutions to such groups.

In this hypothetical situation, the exclusive or primary reliance upon systemic foraging strategies will require individuals to ignore and by-pass potentially vast quantities of food which can be predictably harvested at little energy cost to the forager. Instead, such a group of foragers will necessarily have to expend significant portions of its total energy budget on meeting the systemic requirements of its subsistence—to the detriment of default investments into human biomass and reproduction.

By contrast, a small-scale society focused primarily or exclusively on individual foraging strategies for its subsistence in this environment will have very low energy costs associated with subsistence activities, while potentially accessing a locally plentiful, varied, and reliable source of food. As with all foraging small-scale societies, the default investment of available energy will be channeled into human biomass and reproduction. Given comparable environments, groups which are principally or wholly reliant upon individual foraging strategies will reproductively outstrip groups based on systemic foraging in this scenario. Thus, in the (admittedly special) case of foraging populations faced with environments in which competition for resources from other human foragers is not a factor, systemic foraging techniques are not likely to survive beyond a few generations as systemic foragers become absorbed by the rapidly-growing and more numerous individual foragers. Ultimately, systemic foraging may be able to support higher population densities in certain environments; but individual foraging strategists will have an initial reproductive advantage which (if sustained) can be overwhelming.

Whereas systemic foraging may permit access to more concentrated (larger, more numerous) types of resources, these foraging strategies also require considerably more energy investments from small-scale societies. Specialized technologies used in harvesting and storage, organized population movement or transport of materials, as well as specialized socioeconomic organization, and the gathering and dissemination of pertinent and updated environmental information may all be required in successfully accessing more concentrated energy sources (see section 3.2). Additionally, some forms of systemic foraging (particularly hunting of larger game) have built-in uncertainties related to search time and prey encounter rates which can result in recurrent and unpredictable net energy losses that may not be easily absorbed by small, localized populations.

In the case of the initial human populations of the Americas, we may note that two additional factors may have militated against the introduction and continued use of systemic foraging strategies. First, the coastal adaptations of the parental populations represent relatively generalized forms of subsistence whose success is based in part on low dependency ratios derived from individual foraging throughout the demographic spectrum. Second, the proposed time of entry of the initial populations corresponds to a climatically stable (section 4.4.3) portion of the Wisconsinan in which fine-grained diversified ecosystems favored ecological generalists rather than specialists. To put it simply, and in light of these projections, the first Americans brought a generalized foraging strategy to a stable set of environmental conditions which strongly favored generalists (i.e., individual foraging strategists).

Building on these premises, the net population growth during the initial spread and diversification/adaptation of populations in the

New World was probably invested in population fissioning and emigration into new habitats. Some of the resultant local populations would have thrived, while others ultimately became extinct. As some local habitats continued to support populations over a number of generations, and opportunities for easy emigration gradually decreased, population numbers stabilized while societies continued to maintain themselves through individual foraging strategies. Population equilibrium values based on individual foraging strategies were gradually attained in this manner.

This model output attributes slow growth or demographic stability among generalized hunter-gatherers to the fundamental biological and social differences which separate age and sex classes in these small-scale societies. Men and women of reproductive age are most capable of obtaining food through foraging. Others, including children, are often dependent on adults in their prime for part or most of their subsistence needs. Dependence is most acute by infants and young children. Critical differences between age and sex classes also find expression in the contrasting distances from a central place from which each group can travel during daily foraging.

Pregnancy and lactation by women place heavy biological demands on women's energy budgets. The added energy drain associated with pregnancy weight gain and transporting infants further taxes the energy budgets of reproductive women. These drains on the energy budgets of women are further exacerbated by division of labor practices which might assign to them such tasks as child care, food preparation, gathering of firewood, construction of shelters, and transport of household goods when groups relocate themselves. By contrast, differences in the biological reproductive role of males, coupled with differences in task performance expectations arising from division of labor, mean that males are freed from similar demands on their energy budgets. The practical result of these differences are two-fold.

First, having greater demands placed upon them, women (and their nutritionally dependent offspring) are more subject to the physiological stresses associated with hunger and malnutrition even during times of only minor resource fluctuations. Second, the foraging range of males is larger than the range of children and females with children. Even the relatively modest increase in foraging radius from six to eight kilometers (a one-third increase), will increase the total foraging area utilized by males by over seventy-five percent. Furthermore, since the foraging ranges of all group members overlap, the additional range of males is unique in that it is accessible exclusively by them (see discussions in section 3.3).

Following each relocation of a foraging group, these built-in differences result in a more rapid depletion of the principal resource base on which reproductive women, infants, and children depend. This situation results in nutritional imbalances and energy shortfalls that repeatedly stress physiology. Even when not severe, such stresses will weaken bodily resistence to diseases, resulting in a statistically higher rate of mortality, with the highest incidence occurring among the young (who have the lowest reserves and are thus most vulnerable to nutritional fluctuations). Mortality rates increase in this group as shortages become more acute or chronic.

The endemic nature of the problems resulting from these built-in differences will typically lead to an expectation of high rates of childhood disease and mortality which are viewed within the culture as "normal" albeit unpleasant parts of day-to-day existence. Decisions to intensify foraging, to tap additional resources, or to relocate the group will be made on the basis of the perception of shortages experienced by adults, and quite possibly by adult males (who would be the last individuals to be affected by such shortages).

This system, however unpleasant to contemplate on the basis of our own modern value system, results in a dynamic equilibrium between human population numbers in a given area or habitat, and the critical food resources upon which they depend. Differential shortages by age and sex exert a negative feedback effect on population growth. Short-term cyclic changes in resource density following group relocation lead to increased pressures on local resources, resulting in the expansion of stresses to increasing numbers of the adults in the group. Eventually this situation results in population movement, a shift in prey choices, or both. These responses, in turn, permit preferred prey and habitats to recover, thus perpetuating the cycle of existence.

The operation of this system of relationships will lead to relatively stable population numbers over long periods of time. This system of relationships will reach and maintain equilibrium levels even with different technologies and different resource types. Only significant deviations in environmental productivity or cyclicity of resource availability experienced by a given group, or significant changes in the energy contributions of reproductive-age males to the overall energy budget of the group will alter the outcome of these relationships.

During the saturation phase, many areas reached an ultimate maximum balance between habitat characteristics and the potential capacities of individual foraging strategies practiced by local groups. Demographic growth continued during this period, though gradually slowing as increasing numbers of local populations reached equilibrium levels. It is significant to realize that peak population numbers even at this stage remained considerably lower than the population values attained by later groups of hunter-gatherers practicing combinations of individual and systemic foraging strategies. Although many differences distinguish these two strategies, one of the fundamental differences lies in the considerably greater contribution made by young adult males to the total energy budget of such small-scale societies.

In a continental or hemispheric perspective, equilibrium level refers not only to local equilibrium values, but also to the solution of numerous local problems encountered by foragers which formerly led to the characterization of some habitats as only marginally suitable. The result of these local solutions would yield local subsistence specializations, increased (collective) population numbers on a continental scale, greater diversity of habitats occupied on a permanent or semi-permanent basis, and an increase in the number of culturally distinct groups.

The primary foraging strategy of small-scale societies during this time remained focused on individual foraging—though undoubtedly augmented in some instances by the introduction of techniques and technologies developed by groups living in formerly marginal habitats, designed to access more specialized resources. Some of these techniques and technologies represented departures from traditional individual foraging modes, and thus present the early foundations of later more specialized systemic foraging strategies.

5.5 CRISIS

As used here, a crisis represents a set of changes and restructuring in the dynamic relationships between populations and habitats. These changes restrict growth, and cause the eventual decrease of population size. Long-term and slow changes might not be viewed as a crisis, while short-term and cyclic variations might be resolved through appropriate adaptive strategies. However, widespread and fundamental changes of high intensity will create crisis conditions, especially when demographic equilibrium values have been reached over the broad spectrum of environments.

During the millennia preceding the Late Wisconsinan glacial maximum, the seasonal insolation balance shifted to produce the most clearly equable conditions since the time of initial entry of human groups into the Americas (see Appendix 2). From the standpoint of biotic systems and habitats, this period represents the broadest and maximum extension of fine-grained environments typical of Pleistocene biotic systems. Pertinent to human foragers utilizing individual foraging strategies, these conditions favored the expansion of populations both in numbers and into formerly marginal habitats. As such, this period corresponds to the later stages of diversification/adaptation, eventually leading to saturation when population numbers peaked.

By contrast, the period immediately following the Late Wisconsinan maximum (after 18,000 B.P.) represents a period of increasingly intense crisis (slowly accelerating) for whole biotic systems and for human foraging groups. Guthrie's (1984) explanation of ecological shifts and consequent restructuring of habitats and biotic systems during this period can also serve as a general explanation for the basis of the crisis events impacting human foragers (also see section 4.4.4). The long growing seasons of plants, which permitted the sympatric existence of various species with sequential maturation and reproductive cycles, was replaced by shorter growing seasons and decreased local species diversity.

As a result of the intensification of seasonal differences, latitudinal zonation of species distributions in the non-tropical zones of the Americas became pronounced toward the end of the Wisconsinan. These and other changes (cf. Guthrie 1984; Graham 1979) diminished and ultimately destroyed the capacity of many local and regional habitats to support feeding generalists. These species were replaced by feeding specialists whose adaptations made them well-suited to the monocultural diets available in these newly-created environments. Broadly speaking, climates became more continental, while fine-grained diverse environments gave way to coarse-grained and more simplified ecosystems. Although some of these shifts began shortly after the Late Wisconsinan maximum, it is clear that the most intense and rapid shifts took place between about 15,000 and 11,000 years ago.

As a result of these shifts, human populations formerly in stable equilibrium with environmental conditions would have gradually faced with chronic shortages. Populations would have stopped growing, slowly beginning to decline in numbers. Some marginal habitats occupied during the Late Wisconsinan maximum became incapable of supporting generalized foragers, and probably had to be abandoned. Eventually even some areas which had supported individual forager groups for much longer times became less capable of sustaining viable-sized human populations.

Not all changes would have resulted in gradual decreases of standing human populations. Movement of peoples out of marginal areas into more favorable but already occupied areas containing extant populations stressed probably the resources of these areas, causing some populations to collapse. Elsewhere, as standing populations dwindled, populations disappeared rapidly as mating networks were disrupted. Whole biotic and cultural systems underwent irreversible changes, resulting in the demise of generalists, and the rise of specialists.

5.6 SPECIALIZATION

Specialization on the part of small-scale societies of foragers required the development of both cooperative systemic approaches toward subsistence practices, as well as the development of appropriate effective technologies which permitted access to previously untapped or only marginally utilized resources. There is no inherent reason to assume that all specializations developed during this period were aimed at exploiting a single type of resource or resource category, or that the approaches used were all the same. Neither is there any reason to believe that within local areas or regions there might not have been attempts at accessing a succession of different types of resources chosen on the basis of available technology and changes in relative abundance or ease of access of prey types.

Many of the environmental shifts which are likely to have brought about the crisis of the Late Wisconsinan resulted in the concentration of resources when compared to earlier fine-grained distributions. The variety of plants and animal prey types available in monocultural environments is lower than in diversified ecosystems. Under these new conditions, individual prey encounter rates during foraging would decrease in frequency.

A decreased encounter rate could be compensated by accessing larger prey types through systemic foraging techniques such as hunting of larger game types. These shifts suggest the use of specialized technologies, and in some cases cooperative foraging. Also, given these conditions, the actual foraging encounters would produce more food than a single forager could consume.

Hunting of large prey probably presented greater problems and dangers associated with prey capture. The decrease in foods accessible through individual foraging, and the acquisition of food in excess of the requirements of any one individual, suggests that sharing behaviors would have become increasingly important. These differences probably resulted in significant shifts in subsistence strategies, and in the strengthening of social bonds.

As Pleistocene habitats disappeared and were replaced by more modern environments characteristic of Holocene conditions, many regions began taking on their modern climatic and biotic flavors. These changes did not begin or achieve their modern form everywhere at the same time or at the same rate. Likewise the modal systemic foraging strategies of Holocene hunter-gatherers which we associate with modern natural geographic and biotic provinces did not arise simultaneously everywhere.

Early forms of systemic foraging strategies probably arose thanks to their ability to act as safety nets or hedges against seasonally cyclic shortages, when individual foraging strategies typically failed or were inadequate at best. A prime focus of concern which helped to reinforce the need for systemic types of foraging strategies was the increased incidence of severe winters in the emerging continental climates of higher latitude zones, when large game animals provided a realistic alternative to subsistence based on plants and small animals.

The regular and systematic hunting of large game may have originated as a specialized activity intended to meet subsistence needs during winter months. Groups living in geographically and biotically diversified environments resorted to the hunting of large game as a seasonally specialized activity; while other groups living on the periphery of more uniform ecosystems dominated by big game animals adopted big game hunting as a year-round specialization, and expanded into those habitats. Diffusion of the trait complexes associated with big game hunting was probably aided by the fact that this alternative provided a viable solution to a common problem shared by neighboring populations over very wide geographical areas.

Regardless of whether hunting of large game was a seasonal or a year-round pursuit, the choice of prey types would have depended on the optimal net returns realized, given such factors as human group size, prey encounter rates, known hunting strategies, food processing and storage abilities, and available hunting technology. The present model output views the rise of big game hunting in the newly-formed grasslands of the terminal Pleistocene of North and South America as the result of these processes. As such, these changes in subsistence represent examples of regionally and temporally limited yet highly adaptive cultural responses to large-scale climatic and biotic changes at the end of the Pleistocene.

5.7 TERMINAL PLEISTOCENE/EARLY HOLOCENE POPULATION MOVEMENTS

Following the period of initial entry of human populations during the Mid-Wisconsinan (between 50,000 and 40,000 years B.P.) climatic conditions remained relatively constant, with mildly cooler climate maintaining relatively reduced glacial masses until around 25,000 years B.P. (see sections 4.4.1 and 4.4.3). During this interval the processes of entry, dispersal, and diversification/adaptation saw significant population growth, and the occupation of most major habitats in the Americas with a thin but significant veneer of human groups practicing individual foraging strategies with simple technologies. During this same interval, high latitude inland locations characterized by restrictive environments continued to remain uninhabited.

The thin, discontinuous human settlements along the north Pacific rim retained their tenuous hold along the coast. Occasional disruptions occurred, as minor climatic shifts producing temporary glacial advances, or warming trends producing rises in sea levels significantly altered local conditions (see section 4.4.3). The static hold of these populations on the viable, isolated territories along coastal areas prevented any significant population expansion during this period.

During the following ten to fifteen thousand years (between 25,000 and 15,000 or 10,000 years ago) worldwide glacial masses expanded and then collapsed (see sections 4.4.1 and 4.4.3). Long before the ice sheet's western margins flowed into the Pacific, the increased climatic and ecological disturbance caused by these trends disbalanced local conditions along the coast, causing the collapse of coastal populations based on generalized foraging adaptations.

Perhaps some coastal populations continued to survive into this period, especially if some of these groups succeeded in developing more sophisticated technologies and systemic foraging strategies (the Dyuktai of far northeastern Asia [see section 4.5] may represent an inland group of this period which owed its existence to the use of systemic foraging). Eventually, however, the growth of glaciation must have insured that the northwestern coasts of North America became largely uninhabited; while those populations which had made a successful transition to systemic modes of foraging may have moved inland, and perhaps into Beringia. These, or similar (and perhaps somewhat earlier) groups possessing efficient systemic foraging strategies capable of adapting to the harsh continental conditions, spread to occupy the newly-emerged lands of Beringia and the unglaciated interior portions of Alaska.

Following the glacial collapse of the terminal Wisconsinan, vast new areas of land in Canada were opened to colonization. Possessing well-adapted systemic foraging techniques and an efficient technology, inland Alaskan populations moved eastward and southward into western Canada. The spread of these populations into the Yukon and along the eastern borders of the Cordilleran ice sheet represented slow population dispersals in response to improved climatic and biotic conditions in these areas. Climatic amelioration also encouraged the northward movement of populations from south of the continental glaciers. Although this is the region of the so-called "ice-free corridor," the very use of this name implies opportunities if not intent to move through a narrow passage which can only be defined on maps with considerable hindsight and which is contrary to the character of the population movements described above.

South of the ice sheets, native populations underwent a period of major crisis during this time, eventually giving rise in some cases to groups possessing more specialized foraging adaptations. By the time that these specialized foragers to the south had recovered and developed specialized systemic foraging strategies and technologies, Alaskan populations (precursors to the modern Athabaskans) had spread some distance south into the lands newly freed from the ice.

To the east, the retreat of the ice sheets permitted the northward expansion of native populations originating south of the ice sheets. Thus central and eastern Canada were eventually peopled by the northward movement of these peoples. Alaskan populations, on the other hand, moved into northwestern Canada, giving rise to the distinctive Athabaskan linguistic and genetic populations. These movements, together with the subsequent coastal spread of ancestral Aleut and Eskimo populations, complete the major outlines of the early peopling of the Americas.

As indicated earlier, the model output presented in this chapter is derived from the data and principles presented in the preceding two chapters. The contents of this chapter were guided by the assumption that the Americas were initially peopled some time between forty and fifty thousand years ago, by generalized foragers who entered the Americas as a result of the continued spread of coastal adapted populations living on the margins of the Pacific basin.

The model output also projects that the initial founder population was numerically small, and that it brought with it a relatively simple and generalized lithic technology. The mode of subsistence of these groups relied heavily on individual foraging modes, and was initially maritime oriented.

According to the model output, during the subsequent millennia, the initial coastal spread was augmented primarily by a preferential spread along riverine and lacustrine riparian environments. This dispersal pattern eventually gave way to increasing use of non-riparian environments suitable for use by individual foragers. These dispersal patterns eventually led to the occupation of most of the open habitats south of the continental glaciers—eventually resulting in the equilibrium saturation of the available environments suitable for use by groups of primarily individual foragers.

The model output assumes relatively slow population growth rates throughout most of the Pleistocene, though these growth rates are significantly higher than the population growth rates which characterized the rest of the world throughout much of the Pleistocene. The model output also divides the time between the initial entry event and the end of the Wisconsinan into six demographic and cultural stages. These stages are modeled on the basis of projected population growth and dispersal patterns, relative to dynamic interactions between paleoecological conditions, climatic shifts, and dynamically related cultural responses.

All of these projections were formulated on the basis of earlier discussions which purposefully excluded the archaeological data of Proto-American prehistory. These generalized projections will form the basis for the generation of more specific hypotheses to be presented in the next chapter.

Chapter Six

ARCHAEOLOGICAL CONSEQUENCES OF THE MODEL OUTPUT

The previous chapter presented a generalized and comprehensive set of projections concerning the early prehistory of the Americas which forms a starting point for more specific predictions. This chapter selectively examines a limited number of these projections in relation to the actual archaeological record in the Americas.

As noted at the beginning of the previous chapter (section 5.0), not all the propositions of the model output are immediately or directly testable, while in some instances (e.g., submerged sites) we can expect little or no direct archaeological data. In fact, throughout the predictions of the model output there are varying levels of effectiveness at which testing can take place. As a preliminary statement of the model output, these differences are acknowledged, while subsequent discussions will focus on the most immediately testable components of the model output.

Testing of this model 's predictions should proceed at two interrelated but separate levels. The model output predicts a set of broad-scope processes and events (section 6.2, below) which can best be understood in the context of time and space patterning. This is a set of predictions pointing to patterned changing "average" past conditions over vast units of time and space; and as such, it represents the broadest level at which the model output can be tested.

The model output also makes specific predictions about the modal culture-ecologic relationships and internal cultural dynamics of prehistoric (individual forager) small-scale societies. These predictions have a number of more specific consequences, which under various levels of archaeological preservation may be directly tested at specific sites (see section 6.1). These two levels of testing, however, should not be kept completely independent in all instances, since both relate to our total capacity to test the overall model output empirically.

6.1 INDIVIDUAL FORAGING STRATEGISTS: ARCHAEOLOGICAL CONSEQUENCES

One of the principal predictions found in the model output relates to the persistence throughout most of the Wisconsinan of small-scale societies whose subsistence patterns were dominated by individual rather than systemic foraging strategies (see sections 3.1 and 5.4). This type of subsistence strategy has associated socioeconomic consequences which interact dynamically with both human reproductive rates and environmental resource densities, resulting in the maintenance of low equilibrium population densities.

Subsistence strategies based primarily on individual foraging strategies require relatively rich and varied fine-grained environments (more common in the Pleistocene than in the Holocene). The strategy also involves a variable foraging diet composed of a wide variety of plants and smaller animals accessed in small quantities through numerous random encounters by mobile foragers using limited, expedient, and generalized tool kits, with most elements made from softer, non-lithic materials.

Under this strategy, most foods would be consumed on the spot; although some foods would be shared, while others might require additional preparation. Thus, compared to systemic foragers, central place foraging tendencies would be weakened. Base camps would be frequently relocated, while the activities of base camps would emphasize social interactions, information exchange, safety, and shelter, with much less emphasis on subsistence-related activities than among systemic foragers. Overall, these predictions have specific archaeological consequences.

The total pattern is one which would yield diffuse and very low environmental impacts. Base camps characterized by short occupation and light subsistence activity use would leave an ephemeral record which would include little evidence of permanent shelters, tool manufacturing or maintenance, or durable ecofacts such as large animal bones. Most typically, base camps might contain (where preserved) 1) hearths; 2) ecofacts derived from a heterogeneous faunal assemblage composed mostly of smaller-sized animals; and 3) a limited number of lithic materials dominated by expedient and multi-function tool types. Other, considerably more rarely preserved components of base camps might include, features related to habitation structures, seeds and other plant food remains, and non-lithic tools made from organic materials.

Individual foraging strategists have a low reliance on curated tools—most of which would be made of "soft" non-lithic materials. Sources of "softer" raw materials would be more widely dispersed and easily accessible; these materials are generally more versatile for a wider range of uses than stone; and curated tools made from softer materials have lower overall maintenance and replacement costs associated with them. Thus base camps would contain only a few unspecialized lithic tools for manufacturing and maintaining non-lithic implements.

Given the normal cycle of activities characteristic of individual forager strategists, base camps and extraction sites would constitute opposite ends of a smooth continuum in which the most extreme differences at opposite ends would not always be immediately obvious. Concentrations of critical resources such as flakeable stone or seasonally available food types would be of limited significance to individual foraging strategists, prompting only short-lived temporary relocations of base camps. The absence of specialized curated implements, and the ephemeral nature of these sites due to short-term occupancy, help to distinguish these sites from special function sites characteristic of systemic specialists.

In most instances, food extraction activities would be spatially diffuse; and would require the use of few implements. Tools associated with these activities would include a few multi-purpose curated tools, and a larger proportion of expedient tools. Only fixed locations with inherent diachronic importance (such as natural shelters, or outcrops of suitable stone for tool manufacture) would see repeated or more intensive occupation. With these rare exceptions, and because of the dispersed nature of other resources, most locales did not see concentrated, frequently repeated, or prolonged activities. Given natural transformational processes, and the probable emphasis on the use of perishable implements, the modern archaeological signature of such sparsely used sites might appear as a thin lithic scatter lacking diagnostic tool types. Other sites lacking lithic artifacts, or significant natural preservation, might go unrecognized.

Indeed, one of the significant components which must be recognized in any archaeological analysis of prehistoric remains concerns the differential effect of natural transformational processes which can effectively distort the original archaeological record. Natural transformational processes will selectively preserve the expedient stone tools at food extraction sites, and preserve the few large animal bones differentially from small animal bones and plant remains, most of which would not survive. Therefore, unless transformational processes are factored into archaeological analyses, such sites will yield spuriously overemphasize the activities associated with more specialized, systemic foraging. Only sites with exceptional preservation would reveal the true breadth of resources which formed the dominant feature of individual foraging strategies.

Collectively, these predictions describe a generalized or generic archaeological signature characteristic of individual foraging strategists. These are not intended as universal predictions. These predictions simply provide a descriptive base line from which actual individual groups living in their own unique circumstances deviated in various ways. Deviations from these generalized predictions undoubtedly took place, due to both the influence of stochastic local factors, and due to broader regional and historic shifts. The model output presented in the previous chapter proposes a sequence of such larger shifts. The next section discusses some of the potential archaeological consequences of these shifts.

6.2 REGIONAL AND TEMPORAL VARIATION: ARCHAEOLOGICAL CONSEQUENCES

The model output detailed in the previous chapter included a series of projections or processes with distinctive spatio-temporal characteristics. These projected processes may have occurred once or more than once, and the chronology of their occurrence may not be universally consistent. However, we should begin with a set of tentative broad approximations which can serve as a temporal framework for further discussion.

It is proposed that the process of initial entry most likely took place sometime between 40,000 and 50,000 years ago; and was immediately followed by the non-random, preferential spread of populations into optimal habitats. The model output projects that a significant majority of the initial habitats thus occupied consisted of coastal and riparian habitats along major waterways. A fundamental tenet of the model output is that there was a slow and uneven process of population dispersal, effected by the cumulative behavior of multiple local populations, each of which, at any given time, was in demographic equilibrium relative to local conditions.

Given this slow rate of dispersal, the process of population spread throughout the Americas is estimated to have lasted for many thousands of years. During the first portion of this period, environments along coastal zones and along major waterways would have been settled preferentially. Only during the later portions of this period would more terrestrial habitats within the continental interiors, containing smaller waterways, be settled. As a very rough approximation, we may project that this process was completed sometime between 44,000 and 36,000 years ago. At the end of this period the combined population of North and South America numbered less than 10,000 individuals.

Direct archaeological evidence for the Entry period should be extremely rare or difficult to identify due to: 1) low population numbers, resulting in a minimal record; 2) long elapsed time, which has permitted the greatest amount of destruction and distortion of the record by natural transformational processes; 3) age ranges which approach or exceed normal practical limits of radiocarbon dating; and 4) the probability that large portions of the record are now submerged by higher sea levels.

The most likely context within which sites from coastal areas belonging to this period might be preserved would be in the interiors of islands. Here occasional foraging for inland resources might have left a limited record of such activities. Other sites from this time period might be found in areas which contained streams or lakes at the time of the original period of site occupation. The nature of the archaeological record at these sites will reflect the general characteristics outlined in the previous section (6.1), with the most complete record derived at locales which simultaneously provided a combination of several critical resources such as water, natural shelters, flakeable stone, or particular food sources.

It is proposed that the subsequent period (diversification/adaptation) lasted over 10,000 years, until around 25,000 years ago. Localized adaptations took place which resulted in the addition of permanent settlement of territories away from narrow riparian habitats. Riparian and coastal resources continued to be exploited, and permanent water sources retained their perennial importance to foraging groups; but new adaptations focusing primarily or exclusively on terrestrial resources (i.e., a wider array of plants and small land animals) gained considerably in importance. Total hemispheric population size during this period grew to somewhat over 100,000 individuals.

The archaeological record of this period should continue to offer sites along waterways, with the significant addition of inland sites in a wider variety of habitats and ecological zones. The archaeological signature of individual foraging strategies continued to dominate, with minor local additions of new technologies, and local floral and faunal elements. Indications of some types of incipient systemic foraging techniques may occasionally appear as minor components with extremely localized distribution.

During the next six to eight thousand years populations continued to grow and peak in numbers, as additional habitats were made available by the increasingly equable conditions of the Late Wisconsinan maximum (see discussions in section 4.4.2). The patterns of habitat expansion and incorporation of locally abundant resources developed during the preceding period of Diversification/Adaptation were continued, saturating all available habitats by groups still practicing preponderantly individual foraging strategies. This final saturation period may have seen variable population growth rates which slowed as climatic trends peaked and reversed. Maximum total population values south of the ice sheets may have approached or even slightly exceeded 150,000 individuals.

This period should be associated with a more widespread, variable, and somewhat more visible archaeological record. Sites along ecotonal areas, such as major waterways, should provide evidence of more intensive or repeated use of local resources. Marginal habitats should also yield occasional evidence of occupation, with subsistence activities focused on specific and locally limited resources found in limited microhabitats.

Adaptations to less desirable habitats or to increases in population density in more favorable environments will yield an increased incidence of localized systemic foraging approaches. This trend will result in a variety of local solutions leading to an increase in specialized and curated tools, and an intensification of resource extraction practices. Despite these trends, individual foraging strategies still dominated the approach to subsistence encountered at most sites of this period.

During the subsequent period of climatic and ecological change, crisis conditions resulted in the restriction of available resources and habitats, resulting in a net population decline. The shift toward more coarse-grained habitats and increased zonation resulted in localized increases in population density and increased reliance on systemic foraging strategies, while other areas were left largely uninhabited. It is unclear as to how severely population numbers declined during this period, or how quickly crisis conditions arose in particular regions. It is most likely, however, that these trends began slowly at first. Only during the last three or four thousand years of the terminal Wisconsinan (14,000-11,000 B.P.) did these effects become particularly severe.

Although populations declined to some degree during this period, the increased reliance upon concentrated resources (together with more frequent use of systemic foraging approaches) should result in no apparent decrease in archaeological "visibility." In some ways, these trends should make sites of this time period more obvious and recognizable through increased use of curated tools, and repeated or extended use of base camps as logistic (systemic) foraging approaches became more common.

As the severity of the crisis during the terminal Wisconsinan intensified, numerous local solutions involving new prey types and systemic foraging strategies aimed at accessing them were developed. These solutions (representing local and regional specializations) alleviated somewhat the stresses created by zonation and coarser grained environments, but they did not necessarily resolve the problems posed by increasingly severe contrasts between summer/winter conditions. Seasonal movements, scheduling of increasingly systemic strategies, and innovation of storage techniques had to be developed in response to increasing seasonality. Development of these complex strategies took time, so many of them did not appear until the Holocene.

Shifts toward active hunting of larger animals result in more "obvious" sites containing preserved bones and curated tools used in specialized hunting. Natural concentration of large game animals due to increased zonation in the environment further aids in the archaeological "visibility" of these sites. On the other hand, most systemic foraging approaches adopted during the early portions of this period were still largely localized, yielding a highly variable archaeological pattern.

Only at the close of this period (when conditions became most severe) did widespread adoption of particularly efficient systemic foraging strategies take place on a regional basis. The hunting of large game animals, particularly during winter months, might have been used as a strategy both for accessing food during lean times, as well as increasing the net returns from such activities by utilizing the natural freezing process as an effective storage mechanism.

Such a combination of advantages would have insured that the behavioral and technological complex of traits associated with systemic hunting of large game would have spread fairly rapidly to those areas where such behaviors were possible and more efficient than alternative solutions. People in different ecosystems would not adopt these strategies to the same extent and in the same manner. Ecological zones in which large game animals could be accessed regularly and efficiently, in which habitats were less diverse, and in which climatic conditions permitted increased returns from winter hunting would have seen the greatest reliance on such systemic foraging techniques. Elsewhere, these solutions may have been used only sporadically until changing climatic and biotic conditions decreased the frequency of large game to the point where the associated energy returns rendered these strategies ineffective.

6.3 SUMMARY AND PERSPECTIVES

Given the predictions detailed above, we might summarize the archaeological consequences of the model output as follows:

1) The archaeological record of the Americas should contain a series of early sites (dated geomorphologically or chronometrically earlier than the terminal Pleistocene) which are characterized by an overall pattern associated with individual foraging strategists (as described in section 6.1, above). The aboriginal behavior patterns include:

   a) Low subsistence task specializations.
   b) Low seasonal subsistence specializations.
   c) Minimal reliance on storage.
   d) Minimal transport of raw materials.
   e) Relatively high individual and group mobility.
   f) Preferential use of more abundant site-specific local materials.
   g) Preferential use of soft (non-lithic) materials for tool manufacture.

h) Broad-niche adaptation, with emphasis on plants and small game, and opportunistic use of large game.

2) Viewed from an archaeological perspective, this pattern had the following specific consequences for site formation:

a) Numerous "low impact" ephemeral sites of low archaeological "visibility."

b) High proportion of "expedient tools."

c) Low frequency of "curated tools," especially standardized lithic tools.

d) "Curated tools" most typically also generalized, multiple function tools (lithic tools showing variable edges).

e) Few consistently recognizable tool types in assemblages.

f) More uniform (non-contrastive) distribution of tool types over various sites of any one local group (less site specialization evident from tool types—though resource types may vary).

g) Varied ecofacts, most typically characterized by plants and small animal remains.

h) Large game "kill sites" rare or absent—most such animals obtained opportunistically.

i) Large animal bones (when present) constitute tools or raw material for tools, rather than incidental refuse associated with hunting and butchering activities.

j) Highest impact and most predictable sites focused spatially at locations at which permanent or semi-permanent physical features (such as natural shelters, water, or flakeable stone sources) coincide with long-term or seasonally abundant food sources.

3) This general pattern will further be modified by:

a) Natural differences in resource type and concentration available to foraging groups at the time of the original occupation of the site.

b) Low-level functional differences in the activities associated with each site.

c) Local and regional idiosyncratic variation in tool types, corresponding to local cultural variation becoming regionally more consistent toward the terminal Wisconsinan.

d) Transformational processes differentially preserving tools made from different raw materials, and different classes of ecofacts.

e) A broad-scale set of trends related to environmental and demographic parameters as outlined in section 6.2.

4) At present, many of the parameters governing temporal and regional variations (section 6.2) should be viewed as speculative; and the effects which they predict might well lie beyond our current capacity to test them—given the incomplete nature of the currently available archaeological record and the action of transformational processes. Nevertheless, the pattern of regional and temporal variations has been included as a base for future research goals.

Unfortunately, the criteria currently used for defining artifactual status (and therefore validity of data) from prehistoric sites is modeled after the criteria normally associated with systemic foraging. Much the same is true for the expected geographic/geomorphological distribution of early sites. Curated tools, recurrently found in predictable associations with other tool types or recognizable ecofact classes within paleoenvironmental contexts in which specific resource concentrations are expectable, are among the primary criteria used in defining "valid" evidence.

In fact, these criteria are excellent for archaeological definitions of various classes of valid data pertaining to _systemic foragers_; although they fail dismally in predicting the pattern of data recovered from sites containing the cultural remains of _individual foraging strategists_. Individual foraging strategists are much more likely to have used a substantially different technology based on fundamentally different premises, and focusing on resources in different manners and at different locations within original paleoenvironmental contexts. These differences describe fundamentally different classes of phenomena, and the differences needed to identify them archaeologically are so fundamental that they can be said to represent paradigmatic differences.

As a consequence of these differences, the present model and its associated archaeological predictions require a fundamental shift in perspectives. Archaeological sites should be defined on the basis of their own internal data and structure (_sui generis_), rather than on the basis of the presence or absence of specific predetermined _tool types_. The model output further predicts generalized and locally variable activities focused on a diversified resource base accessed with a primarily generalized and expedient technology. In many ways, this pattern is the exact opposite of present archaeological expectations which are aimed at defining the record left behind by systemic foragers.

These changes in perspective are so fundamental to the present model output that the validity of the associated predictions can not be effectively evaluated without adopting these alternative criteria. All subsequent predictions of the model output depend on comprehension and acceptance of the differential consequences separating the archaeological record of individual foraging strategists from systemic foragers. The discussions presented in the preceding chapters laid the foundations for these differences, while the early archaeological record of the Americas, in turn, provides a means of inductively testing some of the associated predictions.

No single work can hope fully to test the validity of this model or its model output. Therefore, the next chapter should not be viewed in this light, but rather as a preliminary example intended to illustrate the potential applicability of this alternative approach. Only numerous detailed tests, beyond reviews of the archaeological data (such as the one offered in the next chapter) will ultimately provide a valid basis from which the validity of the present model and its model output can be reliably evaluated.

Chapter Seven

NEW PERSPECTIVES ON THE EARLY PREHISTORIC RECORD OF THE AMERICAS

In her celebrated analysis, Dincauze (1984:278) professes to succumb ("for purposes of exposition") to the "fascination of the simple question." Simply phrased, this question asks whether there is any evidence to support the proposition that the Americas were peopled before circa 12,000 years ago.

Clearly, this has been a paramount issue in the field—a question which deserves to be addressed. At the same time, I feel that other issues must also be addressed; and that the answer to the "simple question" is not to be found in isolation, but rather as a by-product of the answers which we obtain to some of the other fundamental questions which we should be pursuing with at least comparable if not greater zeal.

Discussions of criteria by which data can validly be marshalled in support of specific arguments (i.e., as evidence) must, however, be preceded by a statement of the intended goals of research. The intended goals of research, on the other hand, become embedded in the criteria which we set up for evaluating the admissibility of data as evidence. Thus, in 1969 C. Vance Haynes proposed the following criteria:

> The primary requirement is a human skeleton, or an assemblage of artifacts that are clearly the work of man. Next, this evidence must lie *in situ* within undisturbed geological deposits in order to clearly demonstrate the primary association of artifacts with stratigraphy. Lastly, the minimum age of the site must be demonstrable by primary association with fossils of known age or with material suitable for reliable isotopic age dating (Haynes 1969:714).

Twenty years later Meltzer (1989:478) noted that other versions of these same criteria have been proposed by various researchers, and that in his (Meltzer's) estimation these criteria were "fairly straightforward." Indeed it would seem so.

If the above-stated criteria or similar criteria were aimed at addressing the "simple question," then we must agree that all the necessary elements are present. A human skeleton or (by proxy) an assemblage of undoubtable artifacts of human manufacture address the presence of humans; while clear association with datable materials predating 12,000 years B.P. completes the requirements. It is also equally clear that having met these criteria, we would be able to conclude that man was indeed present in the New World prior to 12,000 years ago—an intellectual achievement which hardly seems to warrant the enormous amounts of time and resources spent on it by the archaeological community.

Dincauze's review of the state of the "debate" was aimed at addressing the "simple question" in order to "...move the debate off dead-center by showing how the question can be answered, even in difficult situations" (Dincauze 1984:278). This she tried to do by analyzing the logic and styles of argumentation employed in the debate—particularly the fallacies inherent in the argumentation styles of scholars on one side of the debate, namely those whom Dincauze [1984:279] labeled the "proponents" of "Pre-Clovis" occupation of the New World.

My approach, on the other hand, rejects the [assumed] supremacy of the "simple question"; addresses a variety of other issues which I believe are much more substantive; and predicts that the solution to the "simple question" lies in the solution of other issues—such as the ones which I have introduced in this work thus far. In fact, the persistent focus on a chronological Rubicon around 12,000 years ago owes more to archaeological perceptions than to archaeological reality. Without discarding the importance of ultimately establishing a solid chronological framework, archaeologists would be better served by ignoring the "simple question" altogether. This line of reasoning appears to agree with the orientation of a large proportion of the professional literature, and is evident in the comments of outside observers such as Sabloff [1988].

I would further argue that the differences which separate the point of view advocated in this work from the traditional pursuit of the "simple question" are absolute and fundamental. Once one accepts the proposition that the paramount goal of research should lie in the solution of the "simple question," any further debate about "criteria" becomes pointless.

Asking the question lends validity to a perception of a "boundary" from which only members on one side of the "debate" can profit. It is comparable to the classic example of the fallacy of the complex question wherein an innocent man is asked: "Have you stopped beating your wife?" In other words, it assumes that both sides of the "debate" have agreed on the importance of resolving the "simple question." In a sense, the urgency in resolving the "simple question" turns out to be, more appropriately, the fallacy of the complex question [cf. Copi 1982:108].

Accepting the premise that one should debate the "simple question" serves to validate the reality of a perceived chronological Rubicon. This Rubicon, in turn, reinforces the reality and importance of the more recent data, while automatically casting a doubt on anything dating older than around 12,000 B.P. Obviously, this conception only benefits individuals on one side of the "debate" by inherently weakening the opposing point of view; and by focusing attention on the "oldest," "unequivocal," and "well-documented" evidence of human presence in the New World.

Intended or not, these steps characterize the dynamics and outcome of pursuing the solution to the "simple question." Under these rules, the "debate" is illusory; the outcome is fixed; and despite the protests of "conservatives" and "skeptics" professing a desire to end the "debate," it is in their "enlightened" self-interest to maintain the focus of the debate on the "solution" of the "simple question"—even if they are unaware of the logical dynamics of the situation as I have outlined them here.

Accepting the "simple question" as the focus of research into the early prehistory of the Americas is also to accept a simplistic reductionist approach. Such an approach, in turn, invites further reductionist and simplistic criteria. It seems to me that acceptance of this approach, in fact—more than anything else—is the principal source of the problems which have produced the impasse plaguing the field.

By accepting a simplistic perspective, "the" solution to the basic issue can be narrowed to establishing one or two standard, universal criteria to evaluate the validity of a vast and complex archaeological record (e.g., Payen 1982). Furthermore, in deriving these criteria from a potentially unique and different archaeological record, one can devise (consciously or unwittingly) a set of sorting criteria containing a systematic bias unrelated to the basic validity of the data.

The use of indestructible plastic, or plutonium, may be the hallmarks of twentieth century man; but if archaeologists ten thousand years hence use their presence as "the" criteria for human presence, then they will have to conclude that the New World was uninhabited earlier, and that man must have suddenly entered the Americas shortly before the mid-twentieth century. To put it another way, when the differences which separate researchers on two sides of a debate are paradigmatic in nature, then the goals and criteria utilized by each group should be re-examined and restated, before continuing with any further debate.

Validation criteria of data as evidence depends on the intended goals and uses of the data. When the goals of two groups differ, it is a mistake to believe that one side can establish valid criteria by which the data of the other group can be evaluated as evidence. Even with the loftiest of intentions, the potential conflict of interest is too great, and the resultant conclusions would remain forever suspect.

7.1 PERCEIVED CRITERIA OF "PROOF"

Beyond the chronological focus of the "simple question," the most commonly encountered objections to the validity of "Pre-Clovis" data as evidence concerns the human authorship of archaeological materials. Following the criteria established by Haynes (1969), in the absence of a human skeleton, the alternative lies in discovering an archaeological "...assemblage of artifacts that are clearly the work of man" (Haynes 1969:714) [emphasis added].

In this context, it is particularly enlightening to realize that Haynes explicitly states that "for establishing man's presence, the minimum requirements met for the Folsom site still apply for future excavations" (Ibid.) [emphasis added]. Meltzer (1989:478) notes that

> While the criteria as stated [by Haynes and others] seem fairly straightforward, some believe they are vague and in need of clarification (Bray 1988). Others claim the criteria are insufficient to account for a potentially quite different pre-12,000 B.P. record (Bonnichsen 1978:102; Morlan 1987:295); still others argue that those criteria have been met many times over (Bryan 1978:323) [references in the original].

I would argue that, just as in the case of accepting the primacy of the need to address the "simple question," the need to use the Folsom, Clovis, or any other particular site or sites as exemplars of standards of "acceptable" evidence is deceptive, artificial, and unnecessary. The standards of proof of the rest of the archaeological community exist quite independently of such artificial standards; and such artificial standards need not be accepted or applied here, either. Far from pleading the need for special exemptions, this position simply rejects the assumed special status of pre-12,000 B.P. sites. No separate standards are needed beyond those which apply to all other archaeological sites—irrespective of their ages.

It is true that climate, biota, geography, and population densities during the early prehistory of the Americas were significantly different from Holocene times. It is also true that as a result, one would expect to find specific and predictable differences in the resultant archaeological record. Furthermore, it is true that with greater time depth, natural transformational processes would have had more time to alter the nature of the archaeological record of this period. These, however, can be predicted through the use of standard anthropological and archaeological theory whose principles are universal—rather than unique exceptions to the professional standards of the field. The principles and theory remain constant—only the message or content or the archaeological record is different. These differences, on the other hand, are precisely the reasons why archaeologists everywhere continue to excavate sites.

7.2 DATA AND EVIDENCE IN PROTO-AMERICAN PREHISTORY

In her analysis of the state of the "debate," Dincauze (1984:297) also offered the following statement.

> There are theoretical, epistemological, and evidential levels to any successful argument or demonstration. Anyone undertaking to communicate an argument, that is, to share a point of view, must establish that understanding occurs between speaker and hearer at each of those levels. If the parties share a previously established theoretical orientation and epistemological strategies, the argument can move directly to evidence. However, when the fundamental levels are not shared, explication must begin with them. The Pre-Clovis debates have been conducted mainly at the level of evidence. It is clear, however, that more fundamental issues often divide the parties.

I find myself in essential agreement with Dincauze's assessment. It was the need for dialogue about relevant issues at the paradigmatic level which prompted me to publish an earlier article (Alsoszatai-Petheo 1986), and which has guided the rationale of this work.

My assessment of the paradigmatic state of past and current "Pre-Clovis debates" is that they represent a unique, distinctive, and specialized variation originally derived from earlier expressions of the larger paradigm of American archaeology. Past and current "Pre-Clovis debates" are based on a paradigmatic foundation whose principal tenets appear to include:

1) Equation of archaeological data and interpretations derived from material culture remains and site contexts, with acceptable (or even the only acceptable) evidence (other than human skeletal remains) for human presence in the Americas. This tenet effectively restricts the realm of "proof." At the same time, this tenet permits the dismissal of other lines of evidence by labeling them as simply "suggestive evidence."

Past debates also seem to imply that archaeologically conclusive "proof" from early sites should attain the level of "absolute" or "indisputable proof" in order to merit serious consideration. Attainment of this status can be demonstrated by universal acceptance by the vast majority (if not all) of the "qualified experts" in the field.

2) Insistence upon a process of demonstration which begins with inductive data gathering, and proceeds to inductive generalization based on this data base. Truly independent tests of these generalizations are precluded by the exclusion or dismissal of other (non-archaeological) lines of evidence which are viewed as merely suggestive or less than conclusive.

3) Translation of past successes in demonstrating human presence at a given point in prehistory in the Americas into measures and "canons of proof" by which the validity of subsequent data and work are accepted or rejected. No fundamental or broad-based analysis or discussion of the rationale for accepting such "canons of proof" takes place. The operant principle seems to be an insistence on highlighting past successes and asserting that future work must conform to the same approaches and standards. Given the widespread use of inductive generalizations as vehicles for "explanation," the "canons of proof" invariably incorporate assumptions or requirements specific to the data bases of temporally and culturally-specific units which enjoy widespread current acceptance.

4) Insistence on the capacity of a single or a selected few "diagnostic" components of the total data base to resolve much broader questions. Acceptance of this tenet predisposes both sides in the "Pre-Clovis debate" to attempt to settle the issue of human presence by focusing exclusively on artifact typology and attributes, and on the processes and validity of specific chronometric studies, to the exclusion of a wide variety of alternatives.

In contrast to these tenets, this work proposes a series of alternatives which can be summarized as follows:

A) Insistence upon realistic rather than "absolute" standards of "proof."

The search for "ultimate," "absolute" or "indisputable proofs" is a metaphysical goal. This goal is misplaced in scientific fields of inquiry which traditionally thrive on differences of opinion and uncertainty; and which accept the proposition that the explanations proposed for any physical realities at any given time are transitory reflections of the current understanding and data base of the field. Explanations must be continually tested and retested.

The insistence on realistic, rather than absolute, standards of proof does not negate the obligation to evaluate archaeological data from all sites (including "Pre-Clovis" sites) as evidence according to the accepted standards of the field of archaeology. The standards of proof in archaeology vary depending on the _kinds_ of information which a researcher intends to obtain. An archaeologist would demonstrate contact and trade between two groups differently, and using different data, than the approach and data needed to describe the nature and composition of a prehistoric group's diet. By contrast, the standards of proof in archaeology should not be formulated in such a way that the definition of the units of data or the parameters of the data restrict or exclude data which does not conform to predetermined, culture-specific biases. To do so would amount to an artificial manipulation of data which would exclude specific outcomes of research _a priori_.

Therefore, insistence upon realistic standards of proof can be equated with 1) the _rejection_ of the application of "special standards" derived from inductively-generated culture-specific or temporally-specific criteria based on the previous work of any portion of the field of archaeology; 2) the _rejection_ of the evaluation of the validity of data on the basis of its potential to be harmoniously incorporated within current frameworks of culture-historical interpretation; and 3) the _acceptance_ of the broader standards and criteria common throughout archaeology; not just those accepted by some portion of the practitioners of the field.

B) Insistence upon the use of multiple and independent lines of inquiry, beyond traditional archaeological work, to be used aggressively both as alternative sources of data and evidence; and as effective and heuristic mechanisms whereby errors in any single line of inquiry can be identified by comparison to a mutually-consistent alternative conclusion offered by other lines of inquiry. This second tenet represents what might be the most radical departure from the paradigm which has traditionally dominated the course of past and current work.

The practice of archaeological research is typically an exercise in interdisciplinary work. This approach results in the incorporation of data presented by field and laboratory specialists from other disciplines and other sub-disciplines within anthropology. In the past, this approach has also allowed the inclusion of data and arguments from other sub-disciplines and disciplines in the role of "supportive arguments" used to strengthen the essentially independent and _primary_ interpretations derived from the archaeological data.

While such non-archaeological field and laboratory data have been successfully integrated into archaeological interpretations, the primacy of such "archaeological data" has served to exclude the incorporation of the extra-archaeological methods and conclusions of other sub-fields and disciplines on an equal footing with archaeological interpretations. The emphasis on a comprehensive use of independent lines of inquiry advocated by this work (i.e., as discussed in chapter two of this work) represents a significant departure from past attitudes and practices.

The traditional domain of archaeology concerns past _cultures_. The perspective adopted herein does not threaten this domain. However, the structure and composition of American anthropology unmistakably points to the conclusion that we must also consider human biology as part of the totality of the human experience.

An expression of this observation allows us to propose that the study of early or "the earliest" populations of the Americas should not be limited to an understanding of their culture; but rather, it should be focused on the presence and overall nature of these early Americans. In this view, if we are to use the material culture remains of these peoples as evidence of their presence, we must avoid simultaneously assuming that data and evidence from human biology or data about the paleo-environmental context of these populations offered from other disciplines should be subordinated to archaeologically recognized cultural data. After all, if our goal is to discover the presence and understand the nature of past human populations in the Americas, the objects of material culture produced by these peoples in the past can legitimately be viewed as a type of "proxy data" of human presence.

When viewed in this light, it becomes possible to incorporate on an equal footing the data and arguments presented by related fields and sub-fields. Approached from a holistic perspective, the egalitarian inclusion of additional lines of inquiry yields a set of "checks and balances" on the conclusions of any single line of inquiry. In this dialogue, archaeology is most important in that it provides an independent record of concrete empirical details and answers which other fields are not designed to provide. Given the "impasse" within the sub-field, this approach promises an excellent alternative for problem resolution when compared to the now-familiar alternatives.

The contributions of other fields and sub-fields should never be used as substitutes for archaeological data or as substitutes for the normal course of accepted archaeological processes aimed at yielding interpretations of past cultures. These must clearly remain the exclusive domain of archaeology. But insofar as archaeological data and interpretations pertain to even larger issues, I believe that a research strategy modeled along the lines of the interactively interdisciplinary approach characteristic of modern paleoanthropological research is far more desirable than the approach which characterizes the field today.

C) In evaluating the early prehistory of the Americas, allowances must be made for the possibility that the culture and material culture of Pleistocene "Pre-Clovis" peoples differed significantly in degree and in kind from either putative East Asian Upper Paleolithic cultural progenitors, or from the well known terminal Pleistocene and early Holocene cultures of the Americas.

Acceptance of this third point presents the archaeologist with a serious problem. Traditional approaches of archaeology emphasize cultural continuity wherein cultural changes are built upon earlier forms. I do not intend to challenge this basic assumption.

Unfortunately, archaeologists have traditionally been limited in their demonstration of these processes of culture change to the data provided by [often] gradual changes in artifact typology and seriation of artifact types. I believe that this practice is in part a function of the fact that most archaeologically documented cultural change took place in the context of slow to moderate responses to past environmental and population changes. Rapid and drastic changes are not as likely to have left such a record.

By contrast I view the initial entry and dispersal of populations in the Americas—as well as the drastic environmental changes which accompanied the end of the Wisconsinan—as dramatic and temporally concentrated changes. Each period following these events would have been characterized by major departures from previous conditions that elicited fundamental shifts in adaptive strategies which should find expression in the archaeological record.

Such adaptive shifts would yield new technologies and artifact types; new resource foci; new demographic variables; and new geographical distributions of populations. Undoubtedly later cultures did in fact originate by effecting changes in earlier cultures—however, increasing time depth, lower population densities, and the level of our current knowledge of these time periods all conspire against our ability to document in any detail these rapid cultural shifts at this time.

Ultimately it is not sufficient to suggest that such large-scale cultural changes must have taken place in the absence of supporting archaeological evidence. However, I view our understanding of Proto-American prehistory to be in its infancy. At present, as Dincauze (1984:282) has observed, "there is no widespread agreement [among Pre-Clovis proponents] about the critical sites, acceptable age ranges, or the theoretical context within which observations can be defined as data." As noted at the beginning of this section, the first step towards resolution of the problems which have plagued the field must involve the resolution of paradigmatic and theoretical issues.

Hopefully this work will provide a point of departure for these problems while setting the stage whereby incoming field and laboratory data can be evaluated in a new light. By contrast, strict insistence on finding cultural continuities at this point may actually hinder our ability to establish alternative expectations about the nature of the archaeological data which characterize Pre-Clovis sites. These alternative expectations, in turn, must be derived from a less familiar approach which combines a generalized assessment of East Asian antecedents, paleoenvironmental parameters, demographic parameters, and a review of pertinent aspects of the adaptive strategies of small-scale societies.

D) Finally, all aspects—not just a portion—of the patterned data yielded by archaeological sites should be incorporated into evaluation of the validity of the data yielded by a site. Therefore, the validation of archaeological data as evidence depends equally on the assessment of a mutually congruous data base wherein patterns at the levels of artifacts, microscopic wear patterns, artifact classes, ecofacts, chronometric assessments, geomorphology, site distributional data, stratigraphy, and paleoenvironmental data (when available) from sites; coupled with large-scale regional and temporal patterns (if possible) can be used collectively and simultaneously to evaluate the validity of data as archaeological evidence.

7.3 PATTERNING AND VALIDATION CRITERIA

The most fundamental theoretical tenets which permit us to access information about the past involve the acceptance of the propositions that both natural and cultural phenomena are repeatable and patterned, and that the principles governing recurrent patterning operated in the same way in the past as in the present. Physical and biological laws worked the same way in the past as in the present—while the formulation of such laws is made possible by recurrent patterning observed in the present.

By analogy, the fundamental principles governing human biology and the nature of culture (as a universal human phenomenon) have also remained constant from the past to the present. We can also observe that the physical laws and principles which permit specific objects to be used to perform specific functions in the present also operated in the same way in the past.

The hardness, edge angle, size, weight, and edge contour of a modern knife is capable of performing its function in cutting softer material exactly the same way and for the same physical reasons today as in the past. The qualities and relationships of this cluster of attributes form a pattern. This pattern is both functional, within specific parameters, and recurrent in the form of a physical object which can be defined by its particular constellation of attribute clusters. Though functional in its original context, it is its patterning—not its inferred functionality—which helps to define it as a member of a specific class of artifacts. This is the nature of recurrent patterning at the level of individual artifacts.

At a higher level of patterning, it is found that the universal requirements of human cultural existence will be satisfied through a series of recurrent behavioral patterns which will also impact the occurrence, frequency, and spatial distribution of artifact classes. Certain classes of behavior (e.g., extraction activities related to butchering of a carcass for meat) will require the co-occurrence of various objects (i.e., minimally a carcass of meat and bones, and any object endowed with the attributes of a "knife" capable of performing cutting functions).

These co-occurrences may result in the archaeologically observed recurrent and non-random association of objects ("knives" and animal bones) which permit the inference of past butchering activities at those places and times. In effect, cultural site formation processes reflect the last context in which specific objects interacted with humans; while recurrent patterns of object associations reflect recurrent past human behaviors relative to specific classes of objects or groups of associated objects.

By virtue of their location, sites also show geographic or distributional patterns which reflect subsistence, demographic, and environmental realities at the time of their original occupation. The spatial location of sites reflects their original context within larger, patterned networks which are transitory through time. The definition of archaeological data as the remains of human activity relies on all these levels of patterning; and very importantly, it relies on the congruous interaction of all these levels of patterning.

These principles represent the foundations of all archaeology; not just some, or some special category. These principles are also routinely (if only implicitly) used by all archaeologists to distinguish between natural and cultural phenomena. I see no justifiable reason why we should make an exception to employing these principles and this approach in evaluating archaeological data, regardless of its age or source. More than anything else, then, the discovery and documentation of distinctive non-random patterning characteristic of human activity (and expressed at various congruous levels of patterning) should be the basis by which archaeological data are distinguished from other, non-archaeological natural phenomena.

The fact that human behavior is patterned at various levels is fundamental to the approach advocated herein. Furthermore, recognition of human-patterned data as opposed to the patterning inherent in natural processes should be pursued automatically as part of the research strategy at all sites. After all, the ultimate object of archaeological research should focus on culture and human behavior, rather than on the artifacts themselves.

This approach differs from more simplistic orientations in that the focus of research and the criteria for validation rely on the discovery and definition of patterned human activities in archaeological contexts. At this level of research, there are numerous opportunities to test multiple working hypotheses validly—an opportunity which should be vigorously pursued. At this level also, spatial distribution, associations, stratigraphy, and relative chronology are far more important than absolute chronology and broader geographical distributions. Most important at this level of research is the discovery and definition of a total integrated and congruous set of relationships between all levels of patterning. There is no shortcut or simplistic test which can substitute for the discovery and establishment of such a network of patterning in the data.

The "informational content" of different sites will vary. Contrastive behaviors of human groups over time and space, and within the context of different demographic and environmental parameters will produce differences in the "informational content" of different sites and time periods. Variations in availability and choice of raw materials associated with different activities represented at different sites will also impact the nature of the archaeological record recovered from specific sites. Finally, the overall range of variations impacting the character of the hard data will be further impacted differentially by subsequent transformational processes.

Modern archaeologists can not be held accountable for such variations in the nature of the archaeological record. Standards of "acceptability" of data as evidence which incorporate elements dependent on such variations contain naive tautologies at best, which deserve to be identified and rejected. The critical point which must be made is that it is not the nature or quality of the data present in sites which must meet a set of standards for acceptability; but rather, it is the methods and goals governing research which deserve scrutiny and evaluation. Furthermore, in evaluating research methods we must carefully avoid espousing the use of an inflexible set of "validation" criteria, since the nature of what constitutes a valid set of methods is directly dependent on external variables such as the nature of the record, the variables encountered at specific sites, the stated goals, and the implementation of tests relative to specific hypotheses.

7.4 A LIMITED SURVEY OF PLEISTOCENE ARCHAEOLOGICAL SITES

During the last few decades (since the rise of the "Clovis first" construct in the 1950s and 1960s), archaeologists working in North and South America have reported a significant number of early sites which do not conform to the predictions of the "Clovis first" culture-historic construct; and which might therefore be viewed as anomalous, or at least as presenting some anomalous aspects. Compared to the data yielded by early sites which conform to the predictions of the "Clovis first" construct, the broad geographical distribution of these "anomalous" early sites, the varied technical and stylistic attributes of their artifacts, the diversity of cultural activities represented between various sites, and the broad temporal distribution of these data in the late Pleistocene, all present a much more heterogeneous record.

The classification of these sites as "anomalous," and the fundamental need to dispense with such anomalies by means of invoking explicit plausible errors in methodology or interpretation, can both be viewed as predictable behaviors by the scholarly community operating in the absence of an explicitly formulated alternative theoretical model capable of explaining and predicting the data yielded by these early sites. The formulation of such an explicit model together with its associated consequences, and dependent hypotheses (chapters 5 and 6), makes it possible to offer a limited initial demonstration of the articulation of this model's output with typical examples of sites which have traditionally been rejected by scholars using the "Clovis first" model. The remainder of this chapter is devoted to this demonstration.

In attempting this demonstration we must stress from the onset that the structure and predictions of the model were generated independently from the early archaeological record of the Americas. In the same fashion, we must also recognize that the theoretical and methodological premises which guided the collection, selection, analysis, and reporting of the data used in this demonstration, exhibit selective biases (inclusions, omissions, and emphases) appropriate to the theoretical model or orientation which guided their original collection, analysis, and reporting. These selective biases in the treatment of data relative to any theoretical framework are both commonplace and expectable. Theories are selective. This is why there is no such thing as a "theory of everything." On the other hand, these same selective biases result in a non-random selection of the data which is deemed relevant (under a particular theoretical framework) for publication.

The criteria which formed the bases by which individual sites were selected for inclusion in the initial demonstration presented in the remainder of this chapter included both theoretical and practical considerations which should be made explicit at this point. For example, it is very important to recognize that the demonstration presented in this chapter does not compare the predictions of the model presented in this work to the data or the actual early archaeological record of the Americas, but merely to those portions of the record which have made their way into the published literature under the guidance of the methods and expectations of earlier theoretical frameworks. Furthermore, and as a direct result of using these sources of data, the selection of sites for the purposes of this review had to be narrowed automatically to sites for which the published data were at least minimally amenable to achieving a reasonably meaningful set of comparisons to the topics addressed by, as well as the theoretical focus of the present model output.

The selection of sites was also limited to those early sites defined or definable as "anomalous" when viewed from the perspective of the "Clovis first" construct. Although any site and all categories of sites can yield flawed data which may result in anomalies when compared to the predictions of a theoretical model, the mere recognition of the existence of anomalies can not be used automatically to conclude that the data in question are flawed. This conclusion is valid because all theoretical model outputs have a finite explanatory capacity. When this capacity is ultimately exceeded by portions of the incoming data, an explanatory anomaly will result, requiring the modification or replacement of the model or the model output.

The identification of a genuine explanatory anomaly (one possible hypothesis) requires the elimination of other competing hypotheses. Conversely, we can not conclude that specific data are flawed merely because their acceptance would constitute an explanatory anomaly to a particular theoretical model or its output. These conclusions must first be tested empirically (cf. Platt 1964) before they can be accepted. Unfortunately, the archaeological literature detailing the evidence and significance of the data obtained from pre-Clovis sites in the Americas often fails to apply these critical distinctions, or to reduce the results of empirical tests to an unemotionally objective level. Thus the selection of sites "defined or definable as 'anomalous' when viewed from the perspective of the 'Clovis first' construct" requires an evaluation which includes subjective elements.

Still another set of criteria relating to the selection of sites reviewed below required the use of printed primary sources intended to convey and discuss data from specific sites. Whenever possible, preference was given to sites or information reported in widely-accessible publications, while more inaccessible primary references were used as a source of supplemental details. Secondary sources, and synthetic or comparative works were excluded. The intent of these criteria was to include only sites whose primary data could be easily accessed for first-hand evaluation by anyone reading this chapter. These criteria notwithstanding, it must be acknowledged that "easy access" is a relative matter; and in some cases involving sites, rich in detail, which had been documented in less accessible sources, exceptions were made to these criteria.

To summarize, we may note that the intent of this section is not to test in a formal sense the validity of the model output presented in this work, but to provide a preliminary demonstration of its ability to explain and predict the early prehistoric record of the Americas. No attempt is made at this point to provide an exhaustive, all-inclusive survey of all relevant or potentially relevant sites. This is clearly a selective approach. In order to provide this demonstration, the following criteria were applied to the selection of the sites surveyed in this section:

1) Sites included for consideration must have yielded chronometric evidence of Pleistocene age.

2) The data recovered from these sites must have contained significantly anomalous aspects when viewed from the perspectives of the "Clovis first" construct.

3) The anomalies presented by these sites had not been falsified conclusively and comprehensively by direct and relevant empirical tests demonstrating that they were actually the result of bona fide spurious data or invalid interpretations.

4) The published reports contained minimally suitable types and quantities of data to support preliminary meaningful comparisons to the predictions of the theoretical model presented in this work.

5) The literature on which the survey is based (with a few notable exceptions) was composed of fairly accessible sources. All were primary sources.

It must be noted that the criteria employed in the selection of sites for this demonstration is limited to this review. These criteria are not intended to justify or affirm the "ultimate" validity or importance of either the sites which were included or excluded by this survey. As stated earlier, the discussions which follow are intended to provide a demonstration; and not a formal or exhaustive test of the model output. Broad-based formal tests, just as much narrower site-specific tests, will require the formulation and application of different, stringent criteria.

The data from various sites and the discussions which follow have been grouped into several subheadings. Each of these subdivisions, and the predictions which are presented at the beginning of these subdivisions, are based on the model output presented in chapter 5, and on the hypotheses summarized in section 6.3.

7.4.1 ENTRY AND DISPERSAL

Previous discussions in chapters five and six proposed an initial entry period between 40,000 and 50,000 years ago. Although this estimate spans a period of 10,000 years, the time of actual entry south of the glaciated portions of North America is viewed as an entry event by the last splinter group along the Pacific coast. This last splinter group (numbering between 100 to a maximum of 800 individuals) would have been largely restricted to coastal areas.

Using the minimum population density values of 0.005 persons per square kilometer suggested by Wobst (1976) (i.e., a maximum area of 200 square kilometers per person), we can project that the total area occupied by the entry population would not have exceeded 20,000 square kilometers (for 100 individuals) or 160,000 square kilometers (for 800 individuals). In all likelihood the actual area occupied by this population would have been much smaller, given higher population densities, increased ecotonal richness of coastal areas, and linear distribution of resources.

These maximum geographical measures equal 0.05 to 0.4 percent of the total land area of the Americas (see section 4.2 for a discussion of relevant geographic data). Furthermore, given lowered sea levels and a maritime orientation, it is unlikely that direct archaeological evidence will be found for this period. Perhaps the most likely location where such evidence might be found is on present coastal islands. This evidence would reflect atypical inland activities of groups crossing overland between coastal areas.

Following the initial entry event, the dispersal of descendant populations would have continued preferentially along coastal or near-coastal areas, and inland along rivers and lakes, especially at lower latitudes. As a rough estimate, the model output proposes that this period corresponds to sometime around 44,000 to 36,000 years ago.

Population growth during this period is estimated to have risen to a total of 10,000 throughout the hemisphere. Using minimum population density values (as above), it can be estimated that a maximum of 2,000,000 square kilometers were occupied during this period. This area equals about five percent of the land surface of the Americas.

Low impact land use by generalized individual foragers employing expedient tools and artifacts made from perishable materials would have left few archaeological clues to their presence. A limited number of cores, flakes, and simple stone tools, together with occasional hearths, and a broad, eclectic, and locally variable set of ecofacts (where preserved) might be expectable in the archaeological record. Diverse and numerically rich assemblages are unlikely. Megafaunal kill sites containing sophisticated lithic points and recurrent butchering implement types are highly unlikely.

Several potential sites belonging to this period may have been found. However, it is important to approach the record of these sites with some caution. Sites dating at or near 40,000 years approach the practical upper limits of normal radiocarbon dating (beta counting). This limitation means that there is a possibility that some of these dates may represent maximum to infinite dates, rather than actual dates.

7.4.1.1 SANTA ROSA AND THE WOOLLEY SITE

Along the Pacific coast of California the Channel Islands, and particularly Santa Rosa Island, have produced a series of potentially early sites which fit these predicted criteria (Orr and Berger 1966; Berger and Orr 1966; Orr 1968; Berger 1980, 1982; Wendorf 1982). Early surveys indicated the presence of 100 or more possible fire-reddened pits (Orr and Berger 1066:1409, and Orr 1968:67) exposed by erosion. One of the first fire areas discovered in 1946 was associated with a ring of stones. Unfortunately, the face of the sea cliff collapsed before this fire pit could be studied (Orr and Berger 1966:1409).

Until the late 1970s the primary evidence consisted of repeated U-shaped depressions containing charcoal, and outlined by reddened earth. About one third of these depressions produced dwarf mammoth bones (some also contained bird bones) (Orr and Berger 1966:1410). The mammoth skeletons were typically disarticulated by sections, often darkened or charred, at times battered, and occasionally associated with "chipped stones" (Orr 1968:80-85; Orr and Berger 1966:1415). Although some charcoal samples from these pits produced radiocarbon dates as young as 11,300 ± 160 (UCLA-748), most were considerably older—up to an age of >37,000 years ago (UCLA-749) (Orr and Berger 1966:1415; Orr 1968:73).

Rainer Berger investigated a number of these pits, collecting charcoal and bones of extinct dwarf mammoths (Mammuthus exilis) during the 1960s and 1970s.

> Over a period of some 15 years, during which I was privileged to explore Santa Rosa Island, many fire areas were investigated and dated, but any concrete connection between man and mammoth remained elusive. Many of these fire areas are exposed in the face of sea cliffs or canyon walls, often at levels as high as 30 m, that sometimes prove difficult to excavate. Erosion, however, exposes more of these red-baked areas every year and permits their inspection. In addition, some mammoth remains are found on the horizontal surface of the sea cliffs in close association with stone tools; but the question remains whether these surface finds are truly related or merely accidental (Berger 1980:73-74).

During the late 1970s excavations proceeded at the Woolley site on the south side of Santa Rosa Island. The focus of the site was a large hearth (3 meters in diameter) covered by eight meters of overburden and exposed by a naturally cut gully (Berger 1980, 1982). The hearth was in direct association with mammoth bones and about half-a-dozen stone tools. Michael Wendorf, who investigated the fire pits and early archaeological evidence from Santa Rosa island, states that "the stone tools make up something of a butcher's tool kit, with two thick butt choppers, two scrapers, one core scraper, one burin, and one borer" (Wendorf 1982:174-175).

The circular fire-reddened hearth sloped inward to a depth of 80 centimeters. Abundant charcoal was recovered from the hearth. Microscopic examination of the cellular structure of the recovered charcoal samples demonstrated that all sample materials were of biologic origin. Four samples were submitted for radiocarbon dating (Berger 1980:75).

> All the charcoal samples were treated in the laboratory with hydrochloric acid and subsequently with dilute sodium hydroxide to remove both inorganic carbonates and humic acid contamination. After drying, the samples were converted to carbon dioxide and counted in a 7.5 l proportional counter. None of the four samples indicated any measurable radiocarbon activity and were therefore dated as older than 40,000 years, the upper limit for that particular dating method.

> The great antiquity of the Woolley site prompted a careful macroscopic and microscopic analysis of the stone tools and mammoth remains. Most of these core tools are made of substantially-sized pieces of black metamorphic rock not belonging in the fine-grained alluvium beds. The parent rock material, often found on the beach, is due to Miocene volcanism in the Santa Barbara Channel area which modified shales and sandstones. The tools at this site were apparently made very coarsely in an _ad hoc_ fashion. The modifications effected on the cutting edges are clearly visible... Closer examination also reveals tool wear at the cutting edges or points (Berger 1980:75-77).

Whereas these investigations appear to be a step in the right direction, geochemical analyses conducted by Cushing et al. (1986) on similar "fire pits" located on the nearby island of Santa Cruz suggest that the reddened zones at some locales might be the product of groundwater chemistry, and that some materials previously identified as "charcoal" could be the product of low temperature oxidation of plant material. These interpretations may indeed be valid for some locales, especially on Santa Cruz Island, but there is no compelling reason to believe that they apply universally throughout the Channel Islands, nor specifically at the Woolley site on Santa Rosa Island.

Given the questions raised by Cushing et al. (1986), together with a lack of additional, well-controlled, systematic archaeological investigations of other sites similar to the Woolley site, many of the questions raised by the fire pits of Santa Rosa Island remain unanswered. From the perspectives of the model output presented by this work, the archaeological materials recovered, their site contexts, their ages and geographical location, however, all agree with the basic predictions associated with the "dispersal" phase of the model.

7.4.1.2 CHINA LAKE

During the Wisconsinan, many portions of the Basin and Range province contained freshwater lakes often joined to each other by a network of streams. Occasionally, the cumulative excess of water from some of these lakes overflowed into larger rivers which emptied into the sea. Sensitive to climatic fluctuations, and variably influenced by the influx of water from lakes and streams at higher elevations, these fluvial and lacustrine systems left a complex geomorphic record often tied to early archaeological sites.

Correlations between early archaeological sites and extinct lake shorelines have been reported for the San Dieguito Complex by Rogers (1939); Lake Mohave by Campbell and Campbell (1937); Lake Chapala by Arnold (1957); Lake Manix by Simpson (1958, 1960) and Alsoszatai-Petheo (1975); Panamint Lake by Davis (1968) and Davis et al. (1969); and China Lake by Davis (1974, 1975, 1978a, 1978b, and 1986). Although some of these sites represent early Holocene occupation of the region (e.g., Lake Mohave), a number of these sites date to Late Wisconsinan times or perhaps even earlier. One early site, the East

Rim site (associated with Pleistocene Lake Manix), will be discussed in greater detail in the next chapter.

Based on the discovery of stone artifacts and bones of Rancholabrean fauna scattered on surfaces and in shallow deposits along the extinct shorelines of Pleistocene China Lake, Emma Lou Davis began a mapping and excavation program in 1969. Unfortunately, even after over a dozen years of field research, and multiple publications including an edited volume (Davis 1978b), no comprehensive, systematic report has been published.

Davis conducted mapping, trenching, and excavation over large portions of China Lake and its ancient shorelines. Well over 20,000 artifacts and flakes, and hundreds of fossilized Rancholabrean animal bones and fragments were located and recovered. Davis collaborated with a variety of specialists in the natural sciences both in the field and in the laboratory. This multidisciplinary approach was aimed at reconstructing the complex geomorphology, paleoecology, and chronology of China Lake, which formed the physical and paleoenvironmental context of the archaeological materials recovered at various sites around the lake.

Davis' work was aimed at correlating faunal remains and stone tool assemblages to buried and emerging stratigraphic levels exposed by Holocene erosion in the basin. Exposed artifacts were evaluated on the basis of their surface weathering, raw material, workmanship, and relation to geomorphic features associated with radiocarbon dates. Radiocarbon dates on geomorphic features yielded ages dating from the Holocene to over 30,000 years ago (Davis 1974:3-4). Based on these criteria, and by the extension of stratigraphic and geomorphological relationships, Davis and Panlaqui (1978:38-39) proposed a sequence of eight cultural periods stretching from about 45,000 years ago to protohistoric times.

Davis' earliest periods, the "Early and Late Core Tool Traditions" (Davis and Panlaqui 1978:38), were further supported by a uranium series date of 42,350 ± 3,300 years B.P. (Davis 1986:86) obtained on mammoth tooth enamel associated with two worked flakes at the Basalt Ridge locale at the northwest end of China Lake. Artifacts of the "Core Tool Tradition" showed extreme amounts of weathering; and were produced preferentially from chert and fine-grained volcanic rock by coarse, irregular percussion techniques (Davis and Panlaqui 1978:38). Davis lists a tool inventory for this period composed of choppers, chopping tools, large spokeshaves, stubby broad beaks, long, ovate knife/points, cordiform knife/points, large flake tools, pounding /grinding rocks, and possible bone tools (Ibid.).

The relative amount of weathering on these artifacts may have only limited relative chronological utility within the local area, since exposure (vs. burial), topographic position, and wind patterns may all have contributed to the relative degree of weathering of these materials. The preferential use of chert and fine-grained basalt to the exclusion of obsidian (common in later assemblages) may be more significant. Also significant is the relatively unsophisticated flaking techniques and the recurrent generalized artifact types which characterize these assemblages.

These traits, along with the broader geomorphological context of these materials, fit well the predicted characteristics of the Dispersal period of the model output presented in this work. Nevertheless, despite the extensive and careful work conducted to date, much detailed work is still needed to clarify further and confirm the chronology and associations proposed by Davis and her colleagues.

7.4.1.3 PENDEJO CAVE - THE OROGRANDE COMPLEX

Also in the Basin and Range province but east of the continental divide, a new and potentially highly significant early site is currently being investigated by an interdisciplinary team led by Richard MacNeish. Ongoing excavations at Pendejo Cave began in 1990 as an outgrowth of a broader regional program of excavations initiated in 1984 which was aimed at establishing the origin of agriculture in the American Southwest (MacNeish 1992:1-5).

Pendejo Cave is located at the southern end of the Tularosa Basin, near Orogrande, in south-central New Mexico. Several Pleistocene age pluvial lakes have been identified in the Tularosa Basin. The largest of these, Lake Lucero, covered an area of about 466 square kilometers during its highest stand in the late Pleistocene (Morrison 1991:367). The south end of the Tularosa Basin adjoins the Rio Grande near El Paso, Texas (cf. Morrison 1991:354). Pendejo Cave is located approximately forty-five miles north of the Rio Grande, and about three miles east of an ancient lake beach feature left by the now-extinct Laguna del Oro (MacNeish 1992:3).

Initial excavations in 1990 included a trench which exposed twenty-four stratigraphic zones and minimally twenty-eight periods of occupation (MacNeish 1992:6). These stratigraphic zones have yielded over two dozen Pleistocene age radiocarbon dates ranging from 11,500 to >38,100 years ago (MacNeish 1992:29 Figure 8). Zone I contained burned clay from a hearth-like area. This clay included a human finger and palm print. Radiometric dates on charcoal from an adjacent square indicate an age of around 28,000 years ago (MacNeish 1992:12-13).

The two oldest cultural zones were zones N and O. Zone N contained two activity areas which included a hearth outlined by boulders; bones of rabbit, horse, and camel; three pieces of worked bone; micro-debitage; twenty flakes; and thirty lithic pieces identified by MacNeish as tools. According to MacNeish (1992:14-15), about half of the lithic materials are made from imported exotic materials.

Zone O yielded some burned soil associated with lithic pieces identified as eight pebble choppers and four side scrapers showing use wear; and eight flakes. Fourteen of these specimens are made from stone material imported into the cave from the local mountains. Zone O also yielded bones of rabbit, horse, camel, fox, goat, and antelope. This collection of bones contains primarily lower limb bones, while no skulls, scapulae, pelves, or vertebrae were recovered (MacNeish 1992:15).

Two radiometric dates in excess of 30,000 years BP from zone N (one dating to 36,290 years) are both considered to be too young—possibly representing infinite dates. There was insufficient charcoal in zone O for radiometric dating (Ibid.).

The assemblages from Zones N and O have been collectively labeled the Orogrande Complex. Lithic pieces identified by MacNeish as tools include pebble choppers, scraper plane choppers, laterally convex worked unifaces, and laterally concave worked unifaces (MacNeish 1992:17-20). This assemblage, together with the faunal assemblage, apparent age, and geomorphological context of the site would fit well with the model's predictions for the archaeology of the Dispersal period. At present the only source of published data comes from a preliminary progress report (MacNeish 1992). Hopefully many additional questions, including further clarification of the chronology, geomorphological and paleoenvironmental contexts, and the artifact status of the lithic specimens will be clarified when the final site report becomes available.

7.4.1.4 TOCA DO BOQUEIRAO DA PEDRA FURADA I

In 1970 a joint French and Brazilian team began a program of field investigations and excavations of archaeological sites in eastern Brazil near the village of São Raimundo Nonato in the state of Piaui (Guidon 1986; Guidon and Delibrias 1986; Guidon 1987). The surveys yielded over 250 archaeological sites, most of them associated with rock art (Guidon 1987:6). Subsequent excavations took place at two major sites in the region, Toca do Boqueirão da Pedra Furada and Toca do Sitio do Meio. Excavations were temporarily halted at a major rockfall in the latter site; but the site of Toca do Boqueirão da Pedra Furada yielded well stratified cultural deposits containing artifacts, hearths, and internally consistent sequences of radiocarbon dates stretching well into the Pleistocene.

The region of São Raimundo Nonato contains a series of canyons (boqueirãos) carved into a sandstone plateau which lies in an intermediate position between a plain to the southeast, and jagged mountains to the northwest. The region is semiarid caatinga, supporting cacti and thorny shrubs, as well as patches of forest in the deeper canyons. Modern yearly rainfall averages around twenty-five inches (644 mm) (Guidon 1986:160-161; Guidon 1987:6). Although this is comparable to the annual rainfall of Denver, Colorado, or San Francisco, California (cf. Lane 1980:420), the rains are seasonal, falling primarily from November to March. A series of intermittent streams drain the plateau and the canyons, and eventually form a network of tributaries which empties into the Atlantic (Guidon 1986:160; Guidon and Delibrias 1986:770; Guidon 1987:6).

> Apparently, before 11,000 years ago, the climate was more humid, and the countryside consisted of prairies and stands of tropical rain forest. Even today, in deep, narrow canyons that retain moisture during the dry season, we have found typical rain forest plants (Guidon 1987:12).

Under moister Pleistocene conditions the many intermittent streams would likely have been more permanent, while local basins carved in the bedrock would have provided permanent pools of standing water. One such basin is located at the western border of the Pedra Furada site (Guidon and Parenti n.d.).

Excavations at the Pedra Furada site, begun in 1978, continued for more than ten years. The site is located in a large metamorphosed sandstone rock-shelter, 65 feet above the vegetated valley floor, and containing rock paintings on its back wall (Guidon 1987:8). Stone artifacts and hearths in distinct layers are associated with a series of over twenty stratigraphically consistent radiocarbon dates on Pleistocene age charcoal (Guidon 1986:161-167; Guidon and Delibrias 1986:770; Guidon 1987:8; and Guidon and Parenti n.d.:4).

> A carbon-14 date was obtained for one of the hearths and gave the age of 17,000 ±400 yr (Gif-5397). A piece of rock with two red painted lines was found in the same hearth. This pictograph indicates the practice of rupestral art at that time and makes the site of Pedra Furada the most ancient rupestral art site known in America, and one of the most ancient in the world (Guidon and Delibrias 1986:771).

Excavations at the Pedra Furada site were expanded in the late 1980s. Previous excavations on the west side of the rockshelter encountered sloping bedrock, and had ended at a level dated at 31,700 ±830 BP, and 32,160 ±1000 BP (GIF 6653) (Guidon and Delibrias 1986:770). Subsequent excavations in the central portion of the site provided matching levels and carbon-14 dates to those encountered in earlier excavations. These new excavations also revealed four older cultural levels. Two radiocarbon dates from the youngest of these older levels indicate an age in excess of 40,000 years (Guidon and Parenti n.d.).

No plant and animal remains have been recovered in the Pleistocene levels of the Pedra Furada site. "The acid soil, as well as the water that infiltrates along the sloping bedrock, may be responsible for the lack of such remains in the earlier levels" (Guidon 1987:10). The oldest levels produced rock-lined hearths in association with heavy concentrations of flakes and simple stone artifacts. Lithic artifacts were made exclusively from quartz, which occurs on a debris cone east of the shelter, and exotic quartzite. Artifacts include flaked pebble tools, unretouched flakes, and retouched side and notched scrapers (Guidon and Parenti n.d.).

When we compare the paleoenvironmental setting, and the archaeological record presented by the lower levels of Pedra Furada and the other three sites reviewed in this section, we must conclude that the record of these sites precisely match the archaeological predictions of the model output. Simple, generalized lithic artifacts at each site suggest an expedient technology adapted to immediate needs. Some of the lithic items from these sites (notched scrapers, spokeshaves, and scraper planes) suggest production of implements made from perishable materials.

None of these sites can be classified as specialized "kill sites" containing whole carcasses and specialized hunting and butchering implements. Associated ecofacts at the first three sites suggest an eclectic diet. Some of the associated animal bones may in fact represent the result of scavenging and gathering of animal parts for use, rather than as sources of food. These are precisely the traits predicted for Dispersal period sites.

7.4.2 DIVERSIFICATION/ADAPTATION AND SATURATION

The model output predicts that following the initial dispersal of populations, native American populations would have expanded their coastal, riverine, and lacustrine orientation to include a variety of inland and non-riparian ecosystems. During the Diversification/Adaptation period (between 35,000 and 25,000 years ago), population increases would have expanded preferentially in the tropical and semitropical zones of the New World. Population numbers would have continued to grow, reaching a hemispheric high value around 100,000 individuals. This growth would have allowed populations to occupy about 20,000,000 square kilometers, or about 48 percent of the Americas.

Areas previously accessed during the preceding Dispersal period would have seen the most steady increase in population numbers. Archaeologically, some of these areas may produce a record which begins to establish a few regional characteristics. Otherwise, the Diversification/Adaptation period should continue to yield sites of low archaeological "visibility" containing a variety of expedient tools and (where preserved) artifacts made from perishable materials. The pattern of broad and eclectic foraging would have continued during this period, resulting in a broad range of ecofacts—somewhat expanded to include occasional larger game animals.

In the subsequent Saturation period (24,000 to 16,000 years ago), most of the trends initiated in the Diversification/Adaptation period would have been intensified. Populations would have gradually entered and adapted to most of the available ecosystems. Population probably peaked around 150,000 individuals, occupying 30,000,000 square kilometers or approximately 72 percent of the Americas. Some of this expansion in population and territory would have included entry into climatically and environmentally less favorable areas, a development which would precipitate innovation of new technologies.

Although individual foraging may still be considered as the dominant method of subsistence, local intensification in the use of some resources may have given rise to some incipient forms of systemic foraging. Such trends, together with increased population numbers, densities, and broader use of ecosystems, would have resulted in a somewhat more "visible" archaeological record. Diversification of tool kits should be more evident locally, while whole sites and their tool inventories should begin to reflect contrasting local adaptations over broader geographical regions.

7.4.2.1 MONTE VERDE I

Potential archaeological evidence dating to the Diversification /Adaptation period comes from the lowest cultural level (at the base of MV-7) at Monte Verde, Chile. Unlike other sites such as Pendejo Cave and the Pedra Furada site, which present a more or less continuous sequence of occupation levels, the Monte Verde site yielded evidence of two distinct levels of occupation separated by a 20,000 year hiatus. The upper cultural level (Monte Verde II) was separated from the lower cultural level (Monte Verde I) by an unconformity atop an 80 centimeter sterile zone. The cultural materials associated with the lower (Monte Verde I) level were spatially separated from the later (Monte Verde II) materials, as they were recovered no less than fifty meters away, on the opposite side of Chinchihuapi Creek (Dillehay 1986:336; Dillehay 1987:12; Dillehay 1988:179-181; Dillehay and Collins 1988; Dillehay 1989:18-19; Dillehay and Pino 1989:138-139; and Dillehay and Collins 1991:339).

Although analysis of the earlier (Monte Verde I) materials is still in progress, it appears that this site has yielded evidence for an early occupation period dating to around 33,000 years ago.

> Two concentrations of charcoal specks in hearth-like basins (H-S 1 and 2) and one clay-lined feature (H-S 3) were documented in direct association with the older lithics. These features were located in small, shallow, roughly circular basins ranging in depth from 2 to 3 cm and in diameter from 40 to 70-80 cm... Features H-S 1 and 2 were defined by ash and specks of charcoal scattered in and around them and by burned sand underneath them. Feature H-S 3 is a roughly oval, partially disturbed, clay-lined basin containing specks of burned charcoal (Nothofagus sp.) and unburned and partially burned Juncus sp., an edible reed. Flotation of all feature fill yielded a small quantity of charred and uncharred seeds and root fragments of Juncus sp. (Dillehay and Collins 1988:151).

A charcoal sample of Nothofagus wood from H-S 1 yielded a date of 33,370 ± 530 (Beta-6754), and a sample of carbonized Nothofagus wood from H-S 2 was dated at greater than 33,020 (Beta-7825) years ago (Dillehay 1988:183; Dillehay and Collins 1988:152; Dillehay and Pino 1989:138).

Twenty-six lithics from Monte Verde I included eighteen naturally fractured, culturally used stones; one percussion-split pebble; two edge-battered hammerstones; four worked basalt flakes (two showing striations and macroscopic edge wear); and one 9 cm basalt ovate core with a prepared platform showing at least ten flake scars. Fifteen of the naturally fractured stones had acute edge angles between fifteen and forty degrees, and were directly associated with the hearth-like features (Dillehay 1987:12; Dillehay and Collins 1988:150; Dillehay and Collins 1991:339). Speaking of the naturally fractured stones, Dillehay and Collins (1988:151) state that

> Two of these specimens show clear evidence of micro-use wear as evidence by long, shallow, parallel striations and polish indicative of scraping or cutting plant material or fresh wood. An extensive series of facets are visible on another stone.

Monte Verde is located in south-central Chile, along the banks of the modern Chinchihuapi Creek, fifteen kilometers northwest of the northernmost fjord along the south Pacific coast (Dillehay 1986:320). The Monte Verde I assemblage was recovered along the buried embankment of an ancient stream, next to the modern Chinchihuapi Creek (Dillehay and Collins 1988:150). The Pleistocene age occupation of Monte Verde may have coincided with episodes of local climatic amelioration, as Dillehay and Collins (1988:152) note

> Geological studies in the region show that the 56,000-13,000 yr BP period was characterized by glacial activity, changing climatic conditions, and particularly long cold periods. An interstadial characterized by a warmer but still cool, wet climate might have occurred between 37,000 and 33,000 yr BP. After this period, cold conditions continued until deglaciation began by 15,000-14,500 yr BP. It is possible that the climate impeded human occupation of the area during this hiatus or simply that only two human groups ever chose to settle the site area.

Given the cultural, paleoenvironmental, and chronological data presented by the Monte Verde I occupation, we may suggest that the continued process of population spread and cultural diversification and adaptation to local ecosystems had reached as far as forty-one degrees south latitude along the Pacific Coast of South America by at least 33,000 years ago. Furthermore, the use of naturally fractured stones and simply modified flakes and cores associated with indications of possible wood working all suggest an expedient lithic technology adapted to the production of artifacts made from perishable materials. In all these traits, Monte Verde I represents precisely the archaeological characteristics predicted for the Diversification/Adaptation period of the model output.

7.4.2.2 TOCA DO BOQUEIRAO DA PEDRA FURADA II - IV

Almost four thousand kilometers to the northeast, the Brazilian site of Toca do Boqueirão do Sitio da Pedra Furada (Guidon 1986; Guidon and Delibrias 1986; Guidon 1987) has yielded a continuous stratigraphic sequence of layers containing a dozen stratigraphically consistent radiocarbon dates ranging from 32,160 ±1,000 to 17,000 ±400 years B.P. Guidon and Delibrias (1986:769-771) have summarized the materials recovered from these layers as follows:

> During the most ancient cultural phase, Pedra Furada, large circular hearths are found. They are well built with fallen

> blocks and contain large quantities of charcoal and ash. Lithic industry is abundant and localized around the hearths. The raw materials used for the fabrication of tools were quartz and quartzite, from the pebbles present at the site. Based upon typological and archaeological criteria, this cultural phase has been divided into four stages. In the earlier stage, Pedra Furada I, a lithic collection composed of 560 pieces was found, principally pieces with blunt points obtained by two, three or four convergent flakings, made of pebbles or flakes. Besides these typical tools, pebble-tools [chopping tools and choppers] appear as well as denticulates, burins, notched pieces, retouched flakes and double-edged flakes... In addition to the retouched pieces, the collection includes pebble-hammers, flaked pebbles, flakes and fragments produced by flaking. In this level, fragments of painted rock, fallen from the walls, are perhaps evidence of the ancient practice of rupestral art [pictographs]. Two dates, 31,700 ±830 yr [Gif-6652] and 32,160 ±1,000 yr [Gif-6653] have been obtained from charcoal coming from the first hearth found at the site. The next stage, Pedra Furada II, produced 711 lithic pieces. Eight different hearths which correspond to eight distinct periods of occupation have been dated, giving ages from 23,000 ±390 yr [Gif-6160] to 29,860 ±650 yr [Gif-6651]... Hence the site Pedra Furada was occupied at different times and probably on a temporary basis from 32,000 to 23,000 yr BP. This occupation is supported by the presence of numerous hearths and the abundant evidence of lithic industry.
>
> The characteristic tools of Pedra Furada III are the blunt point made out of pebbles or flakes, along with chopping tools, denticulates, knives, retouched and unretouched flakes, and pebbles bearing flaking scars. This stage was dated at 21,400 ±400 yr [Gif-6160]. A new series of occupations which represents the final stage of Pedra Furada is characterized by side-scrapers and blunt points. The other retouched pieces are small bifaces, denticulates and double-edged tools.
>
> A carbon-14 date was obtained for one of the hearths and gave an age of 17,000 ±400 yr [Gif-5397]. A piece of rock with two red painted lines was found in the same hearth. This pictograph indicates the practice of rupestral art at that time and makes the site of Pedra Furada the most ancient rupestral art site known in America, and one of the most ancient in the world.

An estimated five to six thousand year hiatus separates the most recent Pedra Furada IV materials from the late Pleistocene/early Holocene Serra Talhada complex, above it. In a sequence of radiocarbon dated strata with stratigraphically consistent dates

> ...flint and siltite appear little by little and replace

> quartz, as the raw material for tools, the retouching of the tools is of better quality, and the hearths are of different types. The absence of hearths and charcoal have precluded the dating of the beginning of this phase. An age of between 10,000 and 12,000 years BP can be suggested based on analogy with lithic material found in a test pit in section E of the site which have an age of 10,400 ±180 yr (Gif-5862) (Guidon and Delibrias 1986:771).

Although at present it would be premature to draw any conclusions about the significance of the temporal hiatus between the Pedra Furada IV and Serra Talhada phases, it should be noted that this period corresponds well with the Crisis period of the model output, during which many previously-occupied areas would have been temporarily abandoned.

The Pedra Furada phases of occupation represent a series of recurrent occupations of the site during the Diversification/ Adaptation and Saturation periods proposed in the model output. Throughout this time span the lithic remains obtained at the site continue to point to a simple, expedient technology, highlighted by the specialized activity of rock art production.

The limited variety and simple manufacturing technique evidenced by the artifacts, together with the use of locally occurring quartz and quartzite for tool manufacture, all point to an expedient technology. On the other hand, the presence of "blunt points" (graver/incisors?), denticulates, and several other artifact types suggest activities related to the manufacture of artifacts made from perishable items.

Notably the more recent Pedra Furada phases (i.e., Pedra Furada III and IV) built upon the tool inventory of earlier phases to include side-scrapers, small bifaces, and knives. The addition of these tool types suggests a greater variety of activities, or that more specialized activities took place at the site in later periods. Such a shift also suggests (although it does not necessarily demonstrate) that larger groups of people visited the site during these later periods. The age of the Pedra Furada phases III and IV correspond to the Saturation period; and thus, once more, these archaeological data congruous with the general predictions of the model output for this period.

Four other sites/locales in North America yield evidence for the Diversification/Adaptation period. These include El Cedral, Pendejo Cave, China Lake, and Manix Lake.

7.4.2.3 EL CEDRAL

Multidisciplinary excavations at Rancho La Amapola, near El Cedral, San Luis Potosi, Mexico, took place between 1977 and 1980 (Lorenzo and Mirambell 1986). Located at an elevation of 1700 meters above sea level, the site is situated in a modern semi-desert environment, near a set of springs, in a structural depression which contained a succession of Pleistocene age lakes. The project involved, from its inception, specialists including paleontologists, archaeologists, geologists, soil scientists, and botanists (Lorenzo and Mirambell 1986:107-108).

The initial excavations were aimed at the recovery of paleontological remains from the springs and surrounding areas, under the assumption that these areas had been disturbed, and could not yield reliable stratigraphic information. Subsequent excavations expanded the areas under investigation through careful stratigraphic excavation of adjacent areas (Lorenzo and Mirambell 1986:109).

> A series of 14 radiocarbon dates on organic materials from the complex sequence of lake sediments and spring deposits indicates that the recorded cycle of periodic overflow and periodic drought extended from more than 40,000 years ago until about 2000 years ago (Ibid.).

The approach employed at El Cedral insured the recovery of large quantities of well-controlled paleontological, paleoenvironmental, and geomorphological data which could be used to clarify the context of the cultural remains.

> The excavations at Rancho La Amapola found definite evidence of early man in situ in undisturbed stratified deposits. A discoidal scraper of chalcedony was found in situ in a stratum dated at 33,300 +2700-1800 B.P. A fireplace consisting of a lens of charcoal surrounded by a circle of proboscidean tarsal bones yielded a radiocarbon date of 31,850 ±600 B.P. The modified fragment of horse tibia was found in situ in strata dating older than 15,000 B.P. (Lorenzo and Mirambell 1986:113).

Although the archaeological evidence is extremely limited, the careful field controls, together with the thorough understanding of the geomorphological, chronological, and paleoenvironmental contexts of these materials make these finds highly significant. The use of locally abundant bones to line the hearth is not surprising. Despite or perhaps because of the abundance of evidence for various game animals at the site, it is interesting to note that no hunting implements were recovered.

The single lithic artifact is described as "a discoidal scraper of chalcedony, measuring 49 x 45 x 16.5 mm, made on a primary flake by complete edge trimming on the dorsal surface by direct percussion" (Lorenzo and Mirambell 1986:111). Taken as a whole, the evidence from El Cedral is, once again, in precise agreement with the model output—which predicts the occurrence of isolated, ephemeral archaeological material in bona fide Pleistocene contexts. Following the experimentally generated model of Schaller and Lowther (1969), and given the details of the paleoenvironmental context, it is possible to suggest that the archaeological remains at El Cedral may well represent evidence for scavenging of animal remains left behind by carnivores.

7.4.2.4 PENDEJO CAVE - THE MCGREGOR AND NORTH MESA COMPLEXES

At the stratified site of Pendejo Cave, the Diversification/ Adaptation and Saturation periods of the model output correspond to what MacNeish (1992) has labeled the MacGregor complex and the early North Mesa complex. Over ten stratigraphic layers belonging to this period have yielded stone artifacts, worked bone, hearths, and over one dozen radiocarbon dates. Paleontological remains from these layers include the bones of short-faced bear, lion, bison, horse, llama, antelope, goat, tapir, giant turtle, salamander, weasel, skunk, rodents and birds (Ibid.). Clearly, not all of these are likely to be culturally associated ecofacts. MacNeish (1992:20) describes the MacGregor complex as follows:

> The MacGregor complex seems to be represented by artifact assemblages from zone I, J, K, K1, L, M, M1, and M2, and at present includes 82 classified artifacts as well as 43 chips and a number of pieces of worked bone. continuing from the Orogrande horizon are scraper plane choppers, convex and concave unifaces, and large flat triangular endscrapers. The complex seems to represent a heavy emphasis on unifacial tools. Prismatic Ayacucho unifacial points, which occurred earlier, now become common. Unique to this MacGregor complex are small wedges, burins, and anvils, while chopper wedges and spokeshaves start in this horizon, but last into North Mesa times. About 83 of the lithics, or 65%, are from rocks foreign to the cave and had to be brought into the cave by humans. In addition, there are numerous worked bones, plus three bones with flakes of flint tools imbedded in them. This complex also includes the human palm and fingerprint (in zone I). We might tentatively date the complex between 28,000 and 40,000 BP.

The older of these two dates is an estimate based on the presence of several radiocarbon dates greater than 30,000 years B.P. believed to be infinite dates.

Elsewhere, discussing the North Mesa complex, MacNeish (1992:24) notes

> While my earlier [Orogrande, MacGregor] complexes are represented by few tools, they do seem to hang together, but my upper one with 72 tools from zones D through H is less satisfactory. In fact, I have debated whether it combines one or two complexes... the earlier one having wedge choppers as well as strangled and keeled endscrapers, and the upper one having more scraper planes, serrated tools, and snubnosed scrapers.

The perceived need to divide the North Mesa complex may ultimately correlate with the shift from the Saturation period to the Crisis period of the model output. However, in all fairness to the author, the site is currently still under active investigation; and the analysis and interpretation of the materials summarized here represent merely excerpts from an interim progress report. Nonetheless, the findings at Pendejo Cave are well within the predictions of the model, and the finished report on this site is bound to add enormously to our future understanding of the full range of Pleistocene periods detailed in the model output.

7.4.2.5 CHINA LAKE

A summary of the "Early and Late Core Tool Tradition" at China Lake was presented in an earlier section (7.4.1.2). Davis and Panlaqui (1978:38) had estimated the age range of this tool tradition as spanning between 45,000 and 25,000 years ago. They note a marked change in the artifacts following this period. Generally, the stone technology shifts towards smaller, more refined tools produced through percussion preferentially on jasper and volcanic rock (to the exclusion of obsidian). The specific tool inventory includes fewer, smaller choppers and chopping-tools; spokeshaves of varying sizes; long-borers replacing the earlier broad beaks; end scrapers; crescents; crescentic scrapers; long ovate knife/points; ovoid "biscuit" scrapers; "Valsequillo" knife/points (small cordiforms); pounding/grinding rocks; and possibly some bone tools/weapons (Ibid.).

As I have tried to indicate earlier, despite enormous amounts of field and laboratory work, and despite numerous articles and publications, the systematic study of the sites surrounding China Lake can not yet be considered as complete. Many unanswered questions remain. Having visited a number of these China Lake sites, and having spent a considerable time with the senior author, both in the field and in the laboratory, I feel justified in suggesting that although the broad outlines of the time periods presented here and in section 7.4.1.2 are generally correct, some elements (e.g., crescents and long ovate knife/points) belong to a somewhat later period. Furthermore, much additional fieldwork needs to be done before the details of specific chronological subdivisions will be secure. In the meantime I would suggest that the broad chronological outlines appear to be correct, while the specific subdivisions can only be used as relative guidelines which will hopefully be clarified by future work at China Lake.

7.4.2.6 MANIX LAKE AND OTHER SATURATION PERIOD SITES

Approximately one hundred miles southeast of China Lake lies the Manix Basin, which also contained a large freshwater lake during Pleistocene times. Based on a set of extensive surveys, Ruth Simpson (1958, 1960, 1962) proposed the existence of an extensive set of sites coeval with the lake. She labeled the materials observed at these sites the Manix Lake Lithic Industry. Most of the data from these sites had been obtained as a result of surface finds. As a result, during the next ten years many archaeologists dismissed them, while opinions abounded in the absence of hard data. Only a single cursory attempt to sample these remains (under the assumption that they represented surface finds) was conducted (Glennan 1974, 1976).

In 1972 I conducted controlled, systematic excavations and laboratory analyses at one of these sites (the East Rim site) (Alsoszatai-Petheo 1975, 1978, 1981). This site yielded critical archaeological, geomorphological, and paleoenvironmental data pointing to an occupation of the site during the Wisconsinan maximum. Details of these investigations are summarized as one of the case studies presented in the next chapter.

More recently, as a result of investigations conducted as part of a cultural resource management project, Bamforth et al. (1986) have challenged the validity of Simpson's (1958, 1960) original definition and proposed age of the Manix Lake Lithic Industry. Subsequent publications by Dorn et al. (1986), Bamforth and Dorn (1988), and Dorn et al. (1992) report minimum dates as high as 32,000 years B.P. and 26,000 years B.P. for some selected Manix Lake Lithic Industry specimens using cation-ratio (CR) dating, and accelerator mass spectrometry (AMS) radiocarbon dating.

Whether Simpson's original construct (the Manix Lake Lithic Industry) will stand the test of time will depend on future systematic excavations of other early sites in the basin. At present, however, the data from Dorn et al. (1986), Bamforth and Dorn (1988), Dorn et al. (1992), and Alsoszatai-Petheo (1975), all suggest that these sites contain bona fide early evidence which I feel fits well the predictions of the Saturation and perhaps even the Diversification/Adaptation periods of the model output presented in this work.

Continuing our review of the archaeological evidence presented by sites from the Saturation period, several specific predictions of the model output should be noted. The model output predicts increased population size, wider use of different and sometimes marginal ecosystems, a continued pattern of individual foraging supplemented locally by increasing examples of incipient systemic foraging modes, and a somewhat more "visible" archaeological record between 24,000 and 16,000 years ago.

Of the sites reviewed thus far, Pedra Furada, El Cedral, Pendejo Cave, and China Lake, and the Lake Manix Lithic Industry all contain elements which could be attributed to this time period. Additionally, archaeological evidence dating to this period has been reported from four widely separated sites: Lapa Vermelha IV, Pikimachay, Tlapacoya, and Meadowcroft.

7.4.2.7 LAPA VERMELHA IV

On several grounds, probably the least satisfactory evidence comes from the Brazilian site of Lapa Vermelha IV. Prous (1986a, 1986b), a member of a joint Franco-Brazilian team, reports on excavations conducted in the 1970s near Lagoa Santa, in the state of Minas Gerais. One of the sites excavated by this team, Lapa Vermelha IV, yielded signs of human occupation dated in excess of 25,000 years B.P.

Located above a natural depression which contained a Pleistocene age lake, Lapa Vermelha IV is a limestone rockshelter with a funnel shape which narrows steeply downwards, draining towards its interior through a shaft measuring approximately two meters in width. Although this drainage shaft is unsuitable for human habitation, it contains a fill of sediment redeposited from the higher levels of the rockshelter.

The cemented red clay matrix of the oldest fill in the drainage shaft contained a few (exotic) quartz flakes and cores, a unifacially retouched limestone scraper, and deposits of charcoal which yielded radiocarbon dates of 22,410 ±400 B.P. (Gif-3908), and >25,000 years ago in reverse stratigraphic order (Prous 1986a, 1986b). The reversed dates may well indicate a problem with the older of the two dates which was derived from a small sample, rather than a reversed stratigraphy. Adjoining the cone of cemented red clay and paralleling the full length of the wall of the drainage shaft was a second, separate, younger yellow sandy clay deposit. This second deposit also yielded a sequence of five radiocarbon dates on charcoal in correct stratigraphic order, ranging from 15,300 to 9,520 years B. P.

The yellow sandy clay deposits also yielded quartz flakes and scrapers, along with numerous giant snail shells, bones of a large lake fish (presumably obtained from the nearby lake), and a bone fish hook (Prous 1986a:287). The middle stratigraphic levels of this sequence (approaching the Pleistocene/Holocene boundary) produced giant sloth (Glossotherium) bone; and the skull, long bones, and other bone fragments belonging to a woman (Prous 1986a:287; 1986b:176).

Whereas the remains excavated from the yellow sandy clay levels indicate an interesting diversity of resource utilization belonging to the Crisis period of the model output, the earlier remains from the red clay level (belonging to the Saturation period) are considerably less revealing. Furthermore, the reversed radiocarbon dates of this oldest deposit might suggest a reversed stratigraphy which would render archaeological interpretations of the contextual significance of the artifacts more difficult to support.

7.4.2.8 LONG TERM CHANGES AT PIKIMACHAY CAVE

The site of Pikimachay in the highlands of Peru has yielded a remarkable sequence of seven Pleistocene occupation levels containing minimally two tool complexes named the Paccaicasa and Ayacucho (MacNeish 1971; 1979; MacNeish et al., 1980). These seven levels span the period between 14,000 and 25,000 years ago (MacNeish 1979:1), roughly corresponding to the Saturation and Crisis periods of the model output.

> In or on the seven layers, there were 283 artifacts, 47 pieces of worked bone, 118 flakes, 41 cores, 361 identifiable bones of 18 different kinds of animals, and 270 unidentifiable bone fragments concentrated in 25 nonrandom clusters, some of which were at exactly the same depth... Further, not only are the bones represented not a random sample, but their concentration in clusters is no accident, and, what is more, 40 of them have been worked by man and 7 are unmistakably artifacts (MacNeish 1979:41).

With a few exceptions, including artifacts made from exotic basalt and some flint chips, most of the lithic artifacts were made from the volcanic tuff bedrock of the cave. Thus MacNeish notes that

> Some doubts have been expressed that the 283 stones are artifacts. Carl Phagan, Ruthann Knudson, and Antoinette Nelken-Terner, all lithic experts who worked with these materials, had no doubts that all were tools. Don Crabtree, who saw about 30 items from all layers, was of the same opinion [Ibid.].

In addition to direct analysis of the lithic remains, MacNeish also points to recurrent artifact types, non-random two-dimensional clustering of artifacts within layers, and their repeated association with ecofacts and worked bones and bone artifacts [MacNeish 1971; 1979].

Despite the presence of a "piece of carbon" within an activity area in a dark brown stratum [Zone i][MacNeish 1979:27], and a "circle of rocks associated with reddish soil" (otherwise devoid of charcoal) [MacNeish 1979:39] containing "three possible fire-cracked rocks" and "four blackened bones" [MacNeish et al. 1980:7], hearth features within the seven oldest strata of the site yielded insufficient charcoal for radiocarbon dates. All early dates from the site were obtained from bone samples.

The four earliest stratigraphic zones [zones k, j, i1, and i] contained a tool assemblage termed the Paccaicasa complex (MacNeish 1979:21-29). MacNeish et al. [1980:3] estimate that this cultural phase existed between 25,000 to 15,000 years ago.

> Ninety-six bones of extinct animals (six of them bearing saw or cut marks), about one hundred flakes or core fragments (some of which are of materials foreign to the cave), and seventy-three artifacts (four of which are of material foreign to the cave) were found in the lowest cement-like strata of Pikimachay Cave [Ibid.].

Lithic artifacts from this complex were classified as sidescrapers, scraper planes, various denticulates, denticulated scraper planes, a burin, spokeshaves, double-edged spokeshaves, flake choppers, slab cleaver choppers, core pebble choppers, worked flakes, large utilized flakes, cores, hammer-core choppers, pebble choppers, along with various flakes and pieces of debitage found within various activity areas. MacNeish [1979:21-29] has interpreted these, with ecofacts from sloth, horse, and deer, and the associated worked and polished bones, as evidence of seasonal activities including butchering, wood working, bone working, and flint knapping activities by small microbands. Clear-cut projectile points were absent, although MacNeish et al. [1980:6] consider four pointed, unifacial flakes as possible projectile point candidates.

The subsequent Ayacucho phase is estimated by MacNeish et al. [1980:6] to have taken place between 15,000 and 13,000 years ago. This period (comprising stratigraphic zones h1 and h) is distinguished from the Paccaicasa phase by an increase in the number and types of artifacts, and by the presence of clear-cut stone (ten) and three ground bone triangular projectile points (MacNeish 1979:40-41; MacNeish et al. 1980:6-7).

> A major activity during this period seems to have been butchering because about forty bones from zone h have seemingly cultural modifications, such as scratches, nicks, cuts, and saw marks. The split core pebble choppers (sixteen), cleavers (eight), slab spokeshaves (nineteen), small worked flakes (twelve), large denticulates (thirty-six), large utilized flakes (forty-one), flake spokeshaves (six), pebble side scrapers (twenty), small retouched flakes (three), and double-edged slab spokeshaves (two) in zones h and h1 may have been the tools utilized. Also, tufa [sic] scraper planes (nineteen) and split pebble end scrapers (five) could have been used in butchering, but more likely these and many of the above tools were used in skinning and skin scraping. Further confirmation that the latter activities occurred was the presence of two sloth rib bone fleshers, a split bone awl, and an antler punch. All of these implements could have been an aid in tailoring the cleaned animal skins.
>
> Obviously, the bone implements and bone points are evidence of another activity—bone working—and the so-called side scrapers and burins (eight) could have been the tools used to work bone. The various spokeshave-like and denticulated tools, besides being used in bone working, scraping, and butchering, may have been utilized in wood working, not only for making shafts for the projectile points, but also possibly for other tools (MacNeish et al. 1980:6-7).

From the perspectives of the model output, the Ayacucho phase (as compared to the preceding Paccaicasa phase) represents a period of resource use intensification, along with a rise in systematic hunting activities—both of these trends are predicted for the Crisis period of the model output.

Finally, the Huanta phase (11,000 to 13,000 B.P.), defined from other sites in the Ayacucho basin, completes these trends; and is most closely equivalent to the Specialization period of the model output. During the Huanta phase (which may be represented in zone h at Pikimachay), horse and camelid bones were found in a restricted segment of the site with even more specialized lithic artifacts.

> Unfortunately, only seven artifacts—a fragment of a leaf-shaped bifacial projectile point, four utilized flakes, a crude prismatic blade, and a flat-topped denticulated scraper plane (and/or core)—were found in the zone, along with six animal bones (including two of a rodent that could have been intrusive) (MacNeish et al. 1980:7).

Despite some shortcomings, I feel that the site of Pikimachay represents a stratified site which clearly shows through both its lithic and bone tool inventories the systematic archaeological shifts predicted by the model. Although the last two phases of the site (Ayacucho and possibly Huanta) pertain to later periods predicted by the model, I feel that the unified contrast between all three periods at the site can serve as a point of reference in subsequent discussions, below. Furthermore, although the model predicts a general time framework for these cultural shifts, given variations in local conditions, the subdivisions of the model output should not be applied as rigid time units, but as a continuum of developmental stages whose chronology may be expected to be locally variable.

7.4.2.9 TLAPACOYA

Excavations at the site of Tlapacoya, in the Valley of Mexico, began in 1965. "The interdisciplinary investigation involved the participation of palynologists, paleozoologists, specialists in soils, diatomites, tephras, and geochronological dating" (Mirambell 1978:221). The investigation had been prompted by the discovery of a reddish earth zone in association with charcoal and Pleistocene faunal remains originally exposed during road construction. Several locales were excavated, two of which (Tlapacoya I—including trenches "alpha" and "beta"—and Tlapacoya II) yielded cultural materials in association with radiocarbon dates between 22,000 and 24,000 years B.P.

Initial excavations at Tlapacoya I, trench alpha, yielded a hearth containing

> ...two small flakes of obsidian, a material not present in the nearby surrounding area. Flakes and blades of local stone were also found, as well as a pile of anatomically unrelated bones near the hearth (Ibid.).

Subsequently, several other excavations were conducted. These excavations were helpful in clarifying the geology and stratigraphy at Tlapacoya, but they failed to produce additional early cultural remains.

> Since Tlapacoya I, Alpha, was the only site that had yielded considerable cultural materials, consisting of stone implements and a hearth dated 24,000 ± 4,000 (A-794), new excavations were begun there which turned up two other hearths in soil formed over an ancient beach. As in the first, pebbles and stones were removed, exposing a circular area where abundant ashes and charcoal were found, sufficient for a dating of 21,700 ± 500 years BP (I-4449)... In direct association with the two hearths there were also two piles of various animal bones, many of which were from species extinct in the Mexico Basin; also, some cultural material (Mirambell 1978:224).

Discussing the cultural remains, Mirambell goes on to state

> In the main occupation floor of Tlapacoya I, trench Alpha, three hearths, three obsidian flakes, two small fragments of worked bone, and many [approximately 2,500] andesite flakes and blades were located. The cores from which the artifacts were struck were beach stones; that is, pink andesite of gray lamprobolite, a heterogeneous igneous rock with abundant phenocrysts which chips irregularly. When used for the manufacture of stone artifacts, they appear to be crude. However, this crudeness in not the result of poor technology but of the quality of the raw material...
>
> The obsidian flakes were all by-products of manufacture. One lanceolate obsidian point (33 mm long, 15 mm wide, and 9 mm thick in maximum dimensions) with broken base and tip, and subparallel bifacial retouching. A small notch near the lower third was apparently for hafting. The age of this artifact could not be definitely established since it came from a crotovina that had been filled with an early sediment; it was thereby assigned a minimum age of 15,000 years...
>
> In trench Beta of the same site, a quartz scraper was found. The retouching covered almost the entire periphery, and therefore it was classified as discoidal. This artifact was considerably waterworn, and came from the same stratum as the materials mentioned above, dating to approximately 22,000 years BP (Ibid.).

Excavations at another locale (Tlapacoya II) were begun in 1967.

> Although a trench 60 m long by 1 m wide had been planned, it had to be widened soon after digging was begun because the trunk of a tree (Taxodium mucronatum) was discovered in a basaltic-ash horizon approximately 1 m from the surface reached by the steam shovels, a position which must originally have had 3 m of sediment above it...
>
> The trench revealed a stratigraphic section in the lake-riparian zone which was generally similar, but not identical, to that of Tlapacoya I. In both, there were strata of basaltic and andesitic ash, diatomites, and abundant bony remains but very few artifacts (Mirambell 1978:221, 224).

Among the artifacts recovered at this second locale was a single prismatic obsidian blade struck with a bipolar technique. The blade was recovered from beneath the fallen tree trunk. The tree trunk yielded a date of 23,150 ± 950 years (GX-0950) (Mirambell 1978:228). Correlation of the radiocarbon dates at Tlapacoya I and II has been further supported by obsidian hydration studies which indicate that the amount of hydration of all the tested samples are the same (Mirambell 1978:229).

7.4.2.10 MEADOWCROFT ROCKSHELTER

Meadowcroft Rockshelter is a large sandstone rockshelter in southeastern Pennsylvania, less than one kilometer from the West Virgina border. The south-facing rockshelter overlooks, and is fifteen meters above Cross Creek, a tributary of the Ohio River. Over the last twenty years a wealth of details about this site have been published in a variety of sources (cf. Adovasio et al. 1977, 1978, 1983, 1985, 1988, 1990; Carlisle and Adovasio 1982; Adovasio and Carlisle 1984, 1986, and 1988). As a result, no attempt is made here to summarize the vast quantities of information readily available in the literature.

Of the eleven natural strata at the site, the bottom stratum (stratum I) did not contain cultural materials. Stratum II has been divided into two substrata (IIa and IIb). A heavy episode of roof spalling between these two substrata (dated around 7,000 B.C.) has effectively sealed off the earlier stratum (Adovasio et al. (1977:151). It is the artifacts, features, and early radiocarbon dates from the lower cultural stratum (IIa) which have formed the focus of special interest at this site.

Stratum IIa produced a stratigraphically correct sequence of ten radiocarbon dates in excess of 11,000 B.P. (Adovasio et al. 1988:48; Adovasio et al. 1990:350). The lowermost date of 21,070 ± 475 (DIC-2187), derived from a charcoal concentration, was directly above the Stratum I/IIa contact, and was not associated with any cultural materials. Stratigraphically above this charcoal concentration a carbonized possible fragment of plaited basketry made from a cut bark-like material yielded a date of 19,600 ± 2,400 years B.P. (SI-2060). Seven of the remaining eight radiocarbon dates above this sample yielded dates between 19,100 to 12,800 years B.P. Most of these dates are in clear association with various cultural materials (Ibid.).

Almost from the onset of the publication of these results, critics have objected to the age and interpretations of the site presented by Adovasio and his colleagues.

> Meadowcroft would seem to provide the first hard evidence of pre-12,000 B.P. occupation of North America south of the glaciated areas. The occurrence there of unquestionable artifacts in good stratigraphic context at the base of a multicomponent sequence radiocarbon dated by charcoal in hearths would appear to be unequivocal. After playing a wearisome role as devil's advocate, it was a relief to see such convincing evidence. Upon further consideration, however, there are four aspects of the site that do not leave me completely satisfied. These are (1) a lack of extinct fauna, (2) the occurrence in Stratum IIa of hardwood nuts [e.g., hickory] less than 100 miles from the late Wisconsin ice border, (3) the absence of any paleosol or stratigraphic break in the 4- or 5-millennia hiatus between the date of 7,125 ± 115 B.C. and those around 12,000 B.C., and (4) the lack of Paleoindian and Early Archaic horizons. These factors do not negate the logical interpretations that have

> been made. They are mentioned only as a caution that there may be alternative explanations.
>
> Is it possible that the carbon is contaminated by Paleozoic coal? This is a real possibility, inasmuch as coal deposits occur in the area, and it is being tested by Stuckenrath, who is providing the radiocarbon dates from his superb laboratory at the Smithsonian Institution (Haynes 1977:165).

During the subsequent years a brisk dialogue in published sources has ensued between the critics, and Adovasio, Stuckenrath, and other specialists working with the evidence from Meadowcroft. The published responses by Adovasio and his colleagues not only addressed one at a time each possible objection of the critics from the previously published data, but also involved a vast and detailed set of reexaminations and additional specialized laboratory tests designed to address each of the objections of the critics (Adovasio et al. 1990:348-349).

Despite these vast and credible tests, the critics have continued to raise the possibility of the same objections initially raised by Haynes (1977) (cf. Tankersley and Munson 1992). In raising these objections, Haynes and others have acted appropriately to highlight potential alternative hypotheses which needed to be addressed at Meadowcroft. In reexamining the data and conducting additional critical laboratory tests, Adovasio and his colleagues have amply and explicitly complied with the normal steps of scientific inquiry outlined by Platt (1964) and recommended by Dincauze (1984).

Given the variety of alternative hypotheses suggested by the critics, and given the repeated tests conducted by Adovasio and others, it is not surprising that this process has lasted for well over a decade. What is surprising, unless one takes a cynical view of the entire process, is that the critics (e.g., Tankersley and Munson 1992) continue to raise the same hypotheses as viable possible alternative explanations of the reported data despite published contrary test results.

> All of the recovered data suggests that throughout its history, Meadowcroft served primarily as a locus or station for hunting, collecting, and food processing activities. The predominance of projectile points [in strata above IIa], knives, and scrapers in the lithic assemblage; the relative abundance of food bone; and the remains of edible plants, as well as the general absence of evidence for extensive in situ manufacture of lithic, ceramic, or shell artifacts, strongly support this conclusion (Adovasio et al. 1983:186)

According to Fitzgibbons et al. (1982:96), the artifact assemblage from the lower and middle Stratum IIa (dating between 17,000 and 9,000 B.C.) includes 123 whole and fragmentary tools. These authors go on to say that

> Representative artifacts from these ancient levels include utilized flakes, blades, a variety of bifaces, a very lim-

> ited number of unifaces (including the highly diagnostic Mungai Knife form) and the earliest projectile point type from the Cross Creek drainage, the Miller Lanceolate. A possible fragment of a Clovis fluted point is also associated with the upper middle Stratum IIa deposits (Ibid.).

Discussing the plant and animal remains recovered from these last two levels, Adovasio et al. (1988:52,55) state that

> ...only 11 identifiable bone fragments (and less than 11.9 grams of plant remains) are from the middle and lower Stratum IIa. If all the identified faunal items from these levels represent food remains, which is unlikely, then these human populations exploited white-tailed deer (Odocoileus virginianus) and perhaps much smaller game, at least while in residence at the rockshelter.
>
> The meager floral remains suggest possible utilization of hickory (Carya spp.), walnut (Juglans spp.), and hackberry (Celtis spp.) at the site itself. It is conceivable and likely that these early human populations may have exploited large and now extinct Pleistocene big-game animals, notably mastodon, but there is, unfortunately, no evidence whatsoever of such patterns of predation.

In reviewing these statements concerning the data and significance of these two lower strata, several key points must be highlighted. First, the only potentially "diagnostic" artifacts recovered included the Mungai Knife, the Miller Lanceolate point, and the possible Clovis point fragment. Of these, the last two items were recovered near the top of this sequence, in a living floor dating between 11,300 ± 700 B.P. and 12,800 ± 870 B.P. (Adovasio et al. 1988:52). This dating broadly corresponds to the Specialization period of the model output; and in this light, the lithic remains seem to be entirely appropriate for this period.

The Mungai Knife is illustrated in several articles (Adovasio et al. 1977:153, 1978:164; and Fitzgibbons et al. 1982:97), and is described as a "bilaterally retouched rhomboidal flake 'knife'" (Adovasio et al. 1977:152). The rest of the data recovered from the lower cultural levels of Meadowcroft, together with its age and location, suggest a series of adaptations to increasingly more localized and less hospitable ecosystems. This trend, which may have begun during the Saturation period, appears to have continued at Meadowcroft during the subsequent Crisis period.

The presence of basketry, along with nut shells and hackberry remains, just as the use of deer and smaller game, all point towards a broad and unspecialized form of subsistence in which some local resources may have occasionally been accessed through incipient forms of systemic foraging. These characteristics, in turn, are precisely the types of cultural shifts which are predicted by the model output for the transition between the Saturation and Crisis periods.

7.4.3 CRISIS AND SPECIALIZATION

Following the Wisconsinan maximum, the climates of the Americas underwent increasingly rapid changes resulting in the disruption of many ecosystems both locally, and on a continent-wide scale. From the point of view of human foraging groups, not all ecosystems deteriorated—as, for example, many tropical areas were relatively unaffected—while changes in other ecosystems resulted in new opportunities for foraging groups.

These large-scale environmental changes meant that some areas were abandoned, while other areas were opened to access by groups adopting hunting and other early forms of systemic foraging. These early shifts towards hunting and systemic foraging eventually led to an intensification of these patterns within larger geographical regions during the Specialization period of the model output. This development would have been especially true for areas in which selected resources utilized by local groups became more abundant as a result of climatic changes. Thus, foragers in environments with diverse ecosystems, or incipient hunters near emerging grasslands, would have benefited most directly from the environmental shifts which characterized the end of the Wisconsinan.

During the earlier Crisis period (sometime between about 16,000 and 13,000 years ago) the climatic and environmental shifts of the period would have resulted in a minor overall decline in total population, although local population densities may have increased in some areas as a response to the localized concentration of resources. These population shifts would have intensified towards the end of the Crisis period and during the early portions of the subsequent Specialization period (beginning around 13,000 years ago). The development of viable technologies and hunting strategies during the Specialization period would also have resulted in a return of hunting groups into previously abandoned, now-emerging, highly altered monocultural ecosystems.

The archaeological consequences of these broad shifts would have resulted in 1) increased frequency of specialized and curated lithic and non-lithic technologies associated with a variety of systemic foraging modes, 2) greater "localization" of populations, 3) a higher proportion of sites containing hunting implements in association with the remains of larger animals, 4) a regional intensification of these patterns in favorable areas, and 5) an overall increase in archaeological "visibility" as a result of combinations of these effects.

7.4.3.1 MEADOWCROFT AND WILSON BUTTE

Previous discussions (section 7.4.2.11) introduced the evidence from the middle and upper levels of Stratum IIa at Meadowcroft. Although this site appears to have been only intermittently occupied by foraging groups utilizing local plant and animal resources near the vicinity of the site, and although their activities left only a much compressed set of occupational levels in a limited lower portion of the site, the data provided by Stratum IIa of this site are nevertheless highly significant.

As noted earlier these occupation levels, dating between 17,000 and 9,000 B.C., provide evidence for a gradual shift in adaptive patterns from a relatively unspecialized set of foraging behaviors in the lower strata, to the appearance of specialized hunting implements such as the the Miller Lanceolate point, and the possible Clovis point fragment in a level dated between 11,300 ± 700 B.P. and 12,800 ± 870 B.P. These data agree well with the archaeological trends predicted by the model output, and outlined in the preceding section.

Elsewhere in North America, at the site of Wilson Butte Cave, Idaho, a lava blister on the slopes of a small volcanic vent near the center of the Snake River Plain, similar shifts may have taken place. Here, in the lowest level of a zone of grey/brown sand (labeled Stratum C), a small collection of worked bone and stone artifacts (Assemblage I) is associated with a radiocarbon date (on bone) of 14,500 ± 500 years B. P. (Gruhn 1965).

Wilson Butte Cave is at once both an exciting and frustrating site. Thanks to the dry conditions which prevailed in the region during the late Holocene, the uppermost strata of the cave yielded not only lithic artifacts, but a variety of perishable artifacts. At the same time, these upper levels had been marred by the activities of artifact collectors in the north half of the cave prior to excavation in 1959/60. Excavations in the south half of the cave revealed a sequence of five natural strata of fluvial and aeolian origin (Gruhn 1961).

The small assemblage of artifacts at the bottom of Stratum C included scattered waste flakes and fragments of charcoal, a small basally broken basalt biface, a knife made on a retouched chalcedony blade, a utilized ignimbrite flake later identified by Crabtree (1969) as a burinated flake, and two pieces of worked bone (Gruhn 1961). The bottom of Stratum C also produced bones of various small mammals; birds; and fragments of camel and horse bones.

A separate short report aimed at a technological description of the artifacts in Assemblage I by Don Crabtree (1969) essentially suggests a functional unity oriented towards wood and bone working activities. Based on a detailed analysis of the artifacts, Crabtree suggested that the basalt biface showed evidence of use as a punch or a wedge used for working wood. The chalcedony blade appears to have been used on both sides for whittling wood, while the utilized ignimbrite flake exhibits systematic modifications and re-sharpening on two ends permitting its use as a burin (Crabtree 1969:366-367). Crabtree also goes on to note briefly that one of the pieces of worked bone (an 8 cm x 1 cm rounded limb bone segment) "...has been grooved by a burin-type implement" (Crabtree 1969:367).

Stratigraphically above the zone containing Assemblage I, but still within Stratum C, Gruhn recovered a variety of artifacts (Assemblage II) (Gruhn 1961:47) including well-made bifacial projectile point fragments with affiliations to other similar early Great Basin and Plano age points (Gruhn 1961:52-55). Although this assemblage does not include diagnostic Llano complex projectile points, the succession of materials within Stratum C is, once again, strongly reminiscent of the sequence encountered in Stratum IIa at Meadowcroft.

7.4.3.2 THREE CRISIS PERIOD BRAZILIAN SITES

As mentioned above, the region of São Raimundo Nonato in the state of Piaui, Brazil, was the site of a joint Franco-Brazilian archaeological survey during the 1970s. This survey led to the excavation of two early rockshelter sites, Toca do Boqueirão da Pedra Furada (discussed earlier in sections 7.4.1.4 and 7.4.2.2), and another nearby site, Toca do Sitio do Meio.

Toca do Sitio do Meio also contained a large number of pictographs along its back wall; but unlike the Pedra Furada site, a major rockfall resulted in a significant partitioning of the various portions of the site. Despite these difficulties, Guidon (1986:168-169) has been able to establish a clear set of correlations between the stratigraphic components uncovered in various portions of the site during the 1978 and 1980 field seasons.

The lower sections of Toca do Sitio do Meio above the rockfall contain a sequence of seven stratigraphic units. The top four units are bracketed by radiocarbon dates on charcoal ranging from 12,200 ± 600 years (Gif-5403) at unit XV to 14,300 ± 400 years (Gif-5399) at unit XVIII (Ibid.). The lowermost three units are as yet undated.

These seven stratigraphic units have yielded concentrations of ochre, charcoal, and several hundred lithic specimens. The lithic artifact assemblage is characterized by a predominance of chert flakes, knives, and various scrapers, with an increasing fraction of artifacts made from quartzite, chalcedony, and other silicious rocks in the upper units. Other artifacts include cores, retouched flakes, hammerstones and choppers. Guidon notes that despite the wealth of artifacts recovered from these seven units spanning a period of over two thousand years, very little technological change is evident throughout this period (Ibid.).

To the south, in the Brazilian state of Minas Gerais, lies the site of Lapa Vermelha IV. This site, which as previously described yielded early radiocarbon dates of >25,000 years, and 22,410 ± 400 years, also contained a series of younger deposits with successive dates of 15,300 ± 400 years, 12,960 ± 300 years, 11,680 ± 500 years, and a series of other, Holocene age radiocarbon dates. Although the two oldest dates were obtained from a secondary deposit characterized by reverse dates, the remainder of the radiocarbon dates from this site are in correct stratigraphic order (cf. Prous 1986a, 1986b, and section 7.4.2.7, above).

As noted in an earlier section, a yellow sandy clay deposit within a narrow shaft of this cave site dating to the Crisis and Specialization periods of the model output yielded charcoal, quartz flakes, and scrapers, along with numerous giant snail shells, bones of a large lake fish (from a nearby Pleistocene lake), and a bone fish hook (Prous 1986a:287).

Farther southwest, in the state of São Paulo, lies the site of Alice Boër.

> The Alice Boër site, near to the town of Rio Claro (São Paulo), is situated on a flood terrace about 20 m from the bank of the Rio da Cabeça. The elevation of this terrace

> above running water is less than 10 m. Over an excavation depth of about four m, five lithostratigraphic units have been identified, two of which (layers III and V) yielded lithic artifacts (Beltrão et al. 1986:205).

The first, second, and fourth stratigraphic units at this site were sterile.

> Layer III, with a total thickness of about 2 m, is the richest archaeological layer. It is composed of clayey sands characteristic of a humid climatic phase. Sedimentological evidence suggests that the sedimentation rates toward the base of this layer may have been considerably slower than in the upper levels. The limit between layers III and IV corresponds to an unconformity, the top of layer IV being an erosional surface.
>
> Again, no clear stratigraphy is discernible within layer III. As a consequence, excavation was carried out by horizontal levels of an arbitrary thickness of 10 cm each. Nineteen levels were defined, all of them bearing chert artifacts. Several hundred artifacts were recovered, most of them in levels 7 and 8... Down to level 10, both bifacially flaked stone projectile points and biface tools are present. The last biface fragment was discovered at level 16. Below, only unifacially flaked artifacts were found. Whether the absence of bifacially flaked points below level 10 and bifaces below level 16 are a statistical effect or genuine is not clear because very few tools were found in the lower levels (Beltrão et al. 1986:206).

Because a number of the chert artifacts had been heat treated prior to manufacture, it was possible to obtain a series of thermoluminescence dates directly from the artifacts. The oldest date of 10,970 ± 1,020 years comes from level 8 of Unit III. Charcoal from level 10 has yielded a stratigraphically consistent radiocarbon date of 14,200 ± 1,150 years (SI-1208) B.P. Unfortunately, heat treatment of cherts did not take place before this time; and there are no datable organic materials in the underlying levels and units (Beltrão et al. 1986:206-208).

Unit V at the site is an assortment of river gravels containing occasional crudely shaped lithics which were deposited under a "high energy regime." On the basis of stratigraphic position, Beltrão has proposed that the age of this unit may be somewhere between 20,000 and 40,000 years old (Beltrão et al. 1986:210). Wesley Hurt, who examined the artifacts from Alice Boër, does not accept all the specimens from Unit V as bona fide artifacts. However, he points out, on the basis of several criteria, that he does accept a number of unrolled specimens from the surface of Unit V as genuine (Hurt 1986:216).

Unit III is of particular interest because it shows a gradual transition from simple flake scrapers and knives in its lower levels, to the addition by level 9 of "...two widespread types of bifacially flaked projectile points, one with contracting stems, and the other a leaf-shaped form" (Hurt 1986:218). The gradual appearance of such implements, particularly right above the level dated at 14,200 years B.P., is especially significant when viewed from the perspectives of the model's predictions. In fact, it seems that these Brazilian sites embody the variety of cultural developments predicted for the Crisis period, from fairly stable conditions at Toca do Sitio do Meio, to exploitation of local resources such as snails and fish at Lapa Vermelha IV, to the development of hunting technology using well made lithic projectile points at the Alice Boër site.

7.4.3.3 A MOSAIC OF HUNTERS AND FORAGERS IN THE TERMINAL PLEISTOCENE

The rise of regular systemic foraging involving the regular use of a wide variety of local food sources, or alternatively, the rise of efficient and regular hunting using sophisticated lithic projectile points, marks the terminal portions of the Crisis period, and the beginnings of the Specialization period of the model output. This transition is documented in a variety of settings showing a number of unique local variations on these themes throughout the Americas.

At Pikimachay Cave, Peru, stratigraphic Zone h is dated between 12,000 B.C. and 9,000 B.C. (MacNeish 1979:19). In describing the evidence from this stratigraphic zone, MacNeish (1979:40) notes

> The presence of a blade and the bifacial projectile point with its pressure-retouched edges further emphasized that a new kind of more skillful flint knapping was being undertaken. As in zone h, with its Ayacucho culture, many of the worked materials were from outside the cave, and again, the occurrence of three flakes of Huancavelica obsidian indicated new kinds of wider importations of raw materials for flint knapping.
>
> Other activities were not so prominently evinced, but an Andean bipoint projectile point in conjunction with horse and camelid bones shows that hunting occurred. Unlike previous occupations, hunting seems to have been outside of the cave; the cave was no longer a sloth-kill site. The horse and the camelid bones suggest that the hunting was some sort of specialized herd-hunting, but more evidence is needed from open kill sites before we can understand exactly what sort of herd-hunting occurred.

Evidence from early open kill sites, though not necessarily of herd-hunting activities, comes from two spring sites in the Coro region of western Venezuela. Excavations at the Muaco site yielded bones and artifacts, and radiocarbon dates (on bone) of 14,920 years B.C. and 12,780 years B.C. (Rouse and Cruxent 1963:35-36).

> A fragment of a lanceolate point of the El Jobo type was obtained in situ, among the bones of extinct mammals, and another complete point was found eroding out of the deposit. Also in situ were a piece of plano-convex scraper, like

> those found in all the Joboid complexes, a possible knife, and a number of hammerstones.
>
> This is a typical kill site, where the Paleo-Indians came to hunt because the animals could more easily be approached while they were drinking than at other times. Presumably the projectile points were used to dispatch the animals, which were then cut up with the knife; the skin and bones were cleaned with the scraper, and the bones were broken with hammerstones. The large bones that bear no traces of cutting may have served as chopping blocks, upon which the meat was cut up. As is usual at kill sites, there are no traces of habitation; the Indians apparently cut up the meat in order to carry it more easily to their camps, which were situated elsewhere (Rouse and Cruxent 1963:35).

Unfortunately, modern artifacts were also associated with the ancient artifacts and bones; but fortunately the suggestive evidence from the Muaco site has been corroborated at the Taima-taima site (Ochsenius and Gruhn 1979), located about three kilometers east of the Muaco site in the Coro region of Venezuela. Cruxent's excavation of this site over four field seasons during the 1960s yielded comparable data to those obtained at Muaco, but no intrusive modern artifacts.

> In a basal grey sand unit, among bones of mastodon and glyptodon, he found three fragments of El Jobo points and one uniface knife or scraper, together with rough stones probably used as choppers or pounders and scarred long bones of mastodon probably used as anvils (Bryan et al. 1978:1275).

The basal grey sand unit containing the bones and tools yielded a series of 14 radiocarbon dates ranging from 12,000 to 14,000 years B.P. (Ibid.).

Despite these data, some scholars questioned the validity of the reported associations and dates due to the presence of active springs. As a result, another group of researchers conducted additional excavations at Taima-taima in 1976 with the intent of confirming the associations between extinct fauna and the artifacts, and to further clarify the stratigraphy and age of the site. These excavations yielded the articulated remains of a butchered and partly dismembered juvenile Haplomastodon, in association with artifacts, and abundant sheared wood twig fragments interpreted as the stomach contents of the mastodon (Bryan et al. 1978:1275-1276).

> Two flaked stone artifacts were exposed in situ in direct association with the skeleton of the young mastodon. The midsection of a quartzite El Jobo projectile point was situated within the cavity of the right pubis... A utilized jasper flake was situated within 3 cm of the midshaft of the left ulna. In addition, a rough pointed stone cobble had been jammed in the right acetabulum between the head of the right femur and the acetabulum (Bryan et al. 1978:1275).

Radiocarbon measurements of the age of the stomach contents of the mastodon yielded dates of 12,980 ± 85 years and 14,200 ± 300 years B.P. (Bryan et al. 1978:1276), leading Gruhn and Bryan (1984:131) to conclude that the mastodon kill took place approximately 13,000 years ago. This age is somewhat younger than the dates obtained at Muaco, but the dates are entirely in agreement with the fourteen earlier dates obtained at Taima-taima. Furthermore, the ages and activities indicated at both sites serve to corroborate the trend towards specialized hunting activities predicted by the model, and indicated by the archaeological evidence discovered at other Crisis period sites.

It is important to highlight at this point that the shift towards specialized hunting, and especially towards hunting of large game, was a localized phenomenon. Successful generalized foragers had been present for thousands of years, and most groups during the Crisis period continued to support themselves through the use of broad-spectrum foraging activities supplemented only occasionally by larger game.

Sites such as Meadowcroft, Wilson Butte, Pikimachay, and Alice Boër are especially interesting because they provide a stratigraphic sequence which documents the beginnings of the shift towards a hunting-based economy. The early rise of specialized large game hunting at Muaco and Taima-taima, on the other hand, probably reflect earlier climatic and environmental shifts towards Holocene conditions (cf. Gruhn and Bryan 1984:131) in this area which provided the impetus for the adoption of this specialized subsistence mode.

Elsewhere, such as at the highland Colombian sites of El Abra (Correal 1986; van der Hammen and Correal 1978; and van der Hammen 1978), Tequendama (Correal 1986; van der Hammen and Correal 1978), and at Tibitó (Correal 1981, 1986), in the cool and dry environment of the (2,500 m elevation) Sabana de Bogotá, there is no evidence for the early development of specialized stone projectile points. These sites, yielding early radiocarbon dates of 12,460 ± 160 years B.P. (at El Abra), 11,740 ± 110 years B.P. (at Tibitó), and 10,920 ± 260 years B.P. (at Tequendama) (Correal 1986), suggest that the occupants practiced a generalized foraging form of subsistence not unlike that suggested by earlier Saturation and Crisis period occupations of such sites as Meadowcroft, Wilson Butte, Pikimachay, Lapa Vermelha IV, and Pedra Furada.

The plant remains and animal bones found at the Sabana de Bogotá sites support this conclusion, while the presence of larger animal bones at these sites (such as at Tibitó), suggests that these materials may have been brought to the site for modification and use as bone tools. The continuation of this earlier form of subsistence, once again, may reflect an expression of favorable local environmental conditions in this area, and at other sites such as Monte Verde II (discussed in the next chapter). Such conditions, however, were not available everywhere.

High in the grasslands of the central Peruvian Puna, at the site of Pachamachay, John Rick (1980) found continuous evidence for specialized hunting of various Andean camelids stretching for almost 8,000 years from 11,800 ± 930 years B.P. (UCLA-2118A) into the late

Holocene. Elsewhere, on the plains of south central Patagonia, Argentina, the site of Los Toldos (Cardich 1984; Cardich and Laguens 1984) has yielded a similarly long sequence of occupations by specialized hunters beginning at 12,650 ± 600 B.P. (FRA-98), and ending around 4,500 B.P.

These data are in essential agreement with similar patterns in North America, as at Fort Rock Cave, Oregon, where Stephen Bedwell (1973) found evidence of human habitation dating to at least 13,000 years ago by peoples whose "...cultures apparently were neither exclusively oriented toward big-game hunting or toward gathering, but practiced a balanced type of economy which would endure for thousands of years" (Bedwell 1973:169).

In comparing the materials recovered at Fort Rock Cave to other early archaeological materials in the western United States, Bedwell (1973:170) goes on to say,

> The western pluvial lakes tradition (referring to the lake system to the west along the Cascade-Sierra-Nevada uplift) is typified by the presence of tools having well-controlled percussion flaking, non-stemmed and nonnotched lanceolate projectile points (with round or indented bases), large lanceolate and ovate knives, and substantial numbers of large and moderate-sized scrapers, gravers, and use-worked flakes. While not unique, these attributes predominate in the material culture of this tradition along with similarities in form.
>
> This cultural tradition occupies a region extending from Fort Rock (the northernmost point), south along the Cascade-Sierra-Nevada Uplift in western Nevada and part of northeastern California (in the region of Lake Lahontan) and finally south into the desert areas of southeastern California and pluvial Lake Mohave. Here was an environment which was generally similar throughout. There were certainly area differences, but, the region's numerous lakes made possible a similar environmental adaptation by the inhabitants. So once an economic adaptation had been made which specialized in the exploitation of a lake, marsh, and grassland environment, groups could travel north and south along the Cascade-Sierra-Nevada Uplift and never leave the lacustrine environment which the hundreds of viable lakes at that time provided.

Within this interconnected lacustrine system surrounded by mountains and valley bottoms there existed a rich and diverse patchwork of ecosystems capable of sustaining a cultural pattern based on the practice of a "balanced type of economy"—that is, a generalized foraging and hunting subsistence. Farther east, however, the once diverse ecosystems of the Great Plains had given way to a much more simplified and uniform grassland environment. The first, highly successful residents of this new environment were the specialized big-game hunters, the Clovis peoples. The rise of this culture, just as the rise of their South American counterparts, forms part of a cultural patchwork mosaic of hunters and foragers during the terminal Pleistocene which has been systematically discussed in a recent detailed synthesis by Bryan (1991).

Chapter Eight

TWO CASE STUDIES IN PROTO-AMERICAN PREHISTORY

The preceding chapter presented a set of alternative approaches to the study of early sites in the Americas, and a limited survey of early archaeological sites which support the predictions of the model presented in this work. At the beginning of the survey (section 7.4) it was pointed out that many of the archaeological reports on which the survey was based were produced under the aegis of the old paradigm. This situation meant that although these sites yielded significant archaeological data, the orientation of field and laboratory research, and the nature and content of the archaeological reports for these sites often left significant issues unaddressed. However, having made this statement, I am painfully aware that a critique of current methods can not stand alone in the absence of concrete alternative examples.

The intent of this last chapter, then, is to present an overview of two archaeological case studies which do conform to the approach and standards advocated in this work. Once again, I hasten to state that I do not feel that these are the only two sites which could have been chosen. There are a number of other sites, including some which were not discussed in the previous chapter, which could have been chosen as examples in this chapter. The choice of the two sites presented herein was guided, instead, by a variety of additional significant factors. The two sites chosen for additional discussion in this chapter include Monte Verde II, and the East Rim site.

One of the obstacles which archaeologists constantly struggle to overcome is the selective distortion and destruction of the archaeological record by various transformational processes. Because the destructive effects of natural processes generally increase in direct proportion to the antiquity of archaeological remains, their effects on the oldest sites in the Americas can exceed anything encountered at Holocene sites. Thus, despite the fact that most ethnographically known (and also presumably prehistoric) simple societies utilized wood, bark, plant fibers, leather, bone, antler, or other perishable raw materials to construct the majority of their implements, often the only items left to the archaeologist are the much rarer lithic artifacts.

These problems are not unusual among archaeological concerns. In fact, most archaeologists would be satisfied with a site which yielded non-perishable materials, found in limited numbers, in an undisturbed, stratified, primary deposit—especially when accompanied by such items as ecofacts, features, and datable materials. Such sites pose few unexpected problems requiring the application of special tests or techniques; and can therefore be considered, in a generic sense, acceptable to most archaeologists.

By contrast, the two sites which I have chosen to discuss in greater detail can be considered to be "problem sites" by some. These sites lie at opposite ends of the continuum of transformational processes. As such, each site poses its own special problems; and the approaches used at each site can serve both as specialized methodological examples, and as sources of information about two periods in Proto-American prehistory.

Finally, both of these sites were chosen for closer inspection because both explicitly exhibit an approach towards the entire process of archaeological research (field, laboratory analysis, and interpretation) which can serve as examples of the approach which must be employed at all other Proto-American sites. I am not referring here to the application of sophisticated techniques nor to any aspect of the technical methods employed at these sites. I am speaking instead of a comprehensive approach towards the collection and evaluation of archaeological data which views each site as a single unit, and which seeks to understand the totality of the evidence from such a perspective.

This is not an easy task. It requires not only the collection of geological, geomorphological, botanical, paleobotanical, palaeontological, and other related data—beyond explicit archaeological information—but it also requires that these data should be collected and analysed systematically (both in the field and in the laboratory) under a constant dialogue between a range of nested multiple working hypotheses, and the incoming data provided by the archaeological site.

I hope to demonstrate that when archaeological investigations utilize such a comprehensive and interactive approach, they are able to present a much more complete and reliable reconstruction of past events at the site. This approach also allows the recovery of a wealth of critical information from special sites which standard approaches fail to duplicate. Furthermore, given the variety of special problems (both technical and interpretive) associated with most Proto-American sites, I feel that the application of this comprehensive approach to the investigation of all such sites in the future is imperative.

8.1 MONTE VERDE II - A CRISIS/SPECIALIZATION PERIOD SITE

As stated in an earlier section (7.4.3.3), not all groups living during the transition between the Crisis and Specialization periods adopted a form of subsistence based on specialized hunting associated with technologically complex, curated stone projectile points. This point was demonstrated in the last chapter's review of Crisis/Specialization period sites in Brazil and Colombia.

Apparently at some sites at which climatic and ecological factors remained favorable, some groups retained a variety of older, broad-based forms of foraging. This form of subsistence did not exclude the consumption of meat; but apparently, meat was less important, and where meat was obtained, it may have come from limited hunting of smaller animals, or hunting without the benefit of sophisticated lithic hunting implements. This form of subsistence also emphasized the use of a variety of plant resources. One site which illustrates this persistent subsistence pattern during this period is Monte Verde II.

8.1.1 A BRIEF HISTORY OF THE RESEARCH AT MONTE VERDE

The Monte Verde site is located along the banks of the Chinchihuapi Creek, a small tributary of the Maullin River, 33 kilometers northwest of the town of Puerto Montt, in south-central Chile (Dillehay 1986:320). In 1976 a portion of the small creek shifted its course, returning to an old, filled channel, partially exposing a section of the site (Dillehay 1984:106). Fortunately, the site was discovered later this same year. Two Chilean researchers, Mauricio van de Maele, director of the Museo de Historia y Antropologia, and Carlos Troncoso of the Universidad Austral de Chile, Valdivia, alerted Tom Dillehay, who at the time was head of the program in anthropology at Valdivia, to the presence of the site (Dillehay 1984:106, 1988:178).

> During a visit to the site in 1976, my students and I determined the exact places in the creek banks where bones and human artifacts were being eroded and where Professor Troncoso had excavated a collection of wood and bone. In 1977 and 1978, we conducted a subsurface testing program at the site; we discovered that several elephant bones, split pebbles, and wooden pieces were associated in a patterned spatial distribution and that some of them had been modified (Dillehay 1988:178).
>
> In February, 1979, we returned to the site. The objectives of this field season were rather straightforward: (1) to understand the local geomorphological processes and particularly the events that led to the selection of this location by the site inhabitants, and that developed a peat deposit sealing and preserving the cultural remains; (2) to define more clearly activity areas; (3) to conduct additional surveys along Chinchihuapi Creek and neighboring creeks to search for other sites; and (4) to accumulate modern day plant and faunal collections for comparative analysis (Dillehay 1989a:7).

The excavations conducted in 1978 had uncovered 42 square meters in the culture-bearing zones, and an additional 36 square meters in sterile, natural areas. To this, the 1979 field season added another 95 square meters exposed in the cultural areas, on the north bank of the creek. Geologic columns and additional test pits were also excavated in outlying areas during the 1979 field season (Ibid.).

Although the test pits on the south side of the creek failed to uncover equivalent cultural materials, one of these test pits produced a hearthlike charcoal feature associated with a basalt core at an even deeper level than the materials found on the north side of the creek (Dillehay 1989a:7). These deeper materials were the first indication of the presence of an older, separate site—Monte Verde I (see section 7.4.2.1 for a discussion of this site).

The 1979 field season also saw the beginnings of active participation by a variety of specialists from other disciplines. The project was joined by geologist Mario Pino, whose prior experiences at early sites, and special interests in the Quaternary geology of southern Chile, made him an ideal addition to the team. In addition,

> ...several multidisciplinary specialists were integrated into the project to study the past and present ecology of the area, to analyze the well-preserved organic remains, and to initiate experimental studies in conjunction with archaeological inquiry into the formation processes of the site and nonsites in the local environment (Dillehay 1989a:7).

The early incorporation of specialists from a wide variety of disciplines was an absolute necessity at Monte Verde. Unlike the majority of early sites, in which natural transformational processes have all but completely eliminated the evidence from perishable remains, the Monte Verde site had faithfully preserved unbelievable quantities of both natural and cultural perishable materials. The incredible wealth of such remains at the site, paradoxically, posed a whole new set of problems. As Dillehay (1989a:8-9) puts it,

> ...one of the major difficulties in detecting, defining, and understanding many late Pleistocene sites is our inability to distinguish an artifact from an 'ecofact.' For Monte Verde, we believe that much of the human intervention in the direct contextual association of materials and in the manufacture and/or use of a majority of the lithic, bone, and wood specimens can be verified and explained as a result of human action. However, since a portion of the lithic and wood specimens recovered from the site were often little modified or unmodified from their natural state, it was sometimes difficult to define the ambiguous boundary between some natural and cultural forms...
>
> To better understand how natural forces such as stream deposition, animal burrowing, tree fall, animal trampling, plant growth, and other processes can determine the depositional context, physical appearance, and composition of both perishable and nonperishable materials in a site and to better understand the problem of establishing criteria to distinguish an ecofact from artifact, the interdisciplinary research team was challenged to study Monte Verde in a way more intensive than that accorded most other late Pleistocene sites in the Americas. In an effort to resolve this issue, much of the field, laboratory, and experimental work in 1978, 1979, 1983, and 1985 was oriented toward examination and definition of the composition and distribution of inorganic and organic materials both in the buried cultural context of the site and in the buried noncultural, or natural, context outside the site, along the banks of the paleochannel of Chinchihuapi Creek.

After the 1979 field season, three additional field seasons completed the work at Monte Verde. The brief 1981 field season uncovered the remains of a specialized wishbone-shaped architectural structure. The 1983 and 1985 field seasons continued the active interdisciplinary research described above. In addition to this work, the excavations during the 1983 field season uncovered the major portions of a large residential area at the site. Excavations of this residential area were completed during the 1985 field season, while additional excavations also took place at the deeper, Monte Verde I locale, and at another site located during an earlier survey. Additional laboratory work and analysis have continued since 1985 (Dillehay 1986:323-324, 1989a:10-11).

8.1.2 MONTE VERDE - THE MODERN NATURAL SETTING

Brief introductory descriptions of the natural setting of the Monte Verde site can be found in several publications (Dillehay 1984:106, 1987:8, and 1988:179). Additional and more detailed information is also available about various aspects of the modern natural environment (Ramirez 1989a; Dillehay 1989c, and 1989d). The following description is based on these sources.

Monte Verde is located on a peninsula at 41 degrees south latitude, variably twenty to fifty kilometers from the Pacific Ocean. The site's location places it at the southern terminus of the Province of Llanquihue which extends north to Valdivia (Ramirez 1989a:54). Monte Verde sits within a major physiographic feature of this Province, a long central valley. "The valley runs on a north-south axis and is bounded by a narrow Pacific littoral zone and a low coastal mountain range [the Coastal Andean Cordillera] to the west and the high Andean cordillera to the east" (Dillehay 1984:106).

> The Coastal Andean Cordillera is a low range that does not exceed more than 600 m in height. It is made up of mountains dissected by narrow river valleys cutting transversely across the range. The highest altitude of this range does not extend above the timberline. On the summits there are peat bogs maintained by excessive humidity (Ramirez 1989a:55).

The Province of Llanquihue contains several lakes. Lake Llanquihue drains by way of the Maullin River southward into the Pacific Ocean. The modern climate around Monte Verde is temperate and humid, characterized by moderately warm summers, and cool rainy winters. Temperatures rarely drop to freezing. Annual temperature averages range from twelve to fifteen degrees Celsius (Ramirez 1989a:57; Dillehay 1988:179). "Cloud cover is high and permanent; clear days occur only in summer. The prevailing winds are from the northeast and, moreover, bring rain" (Ramirez 1989a:57).

> Most of the territory of the Province of Llanquihue was totally covered by evergreen forest prior to heavy European colonization of the region in the last century... In addition, there were swamps, streams, lakes, ponds, marine wet-

> lands, dunes, and peat bogs (Ramirez 1989a:58).
>
> A forest made up of a mixture of deciduous and coniferous species covers the region; it supplies numerous varieties of edible tubers, nuts, berries, fruits, and soft and leafy plants abundantly throughout the year. There are also small game, freshwater mollusks, and fish (Dillehay 1988:179).

Thanks to modern human encroachment, the area's fauna has been much reduced. Even so, the modern fauna near the site includes rabbit, ground squirrel, skunk, deer, puma, ocelot, fox, mouse, and other small fur-bearing mammals. Rivers and lake shore environments support beavers and nutria, while the seashore environments produce abundant fish, shellfish, birds, and sea mammals (Dillehay 1989d:85-86).

8.1.3 SITE GEOLOGY AND STRATIGRAPHY

Mario Pino, who conducted the geological field survey in and around the site, defined the objectives of his work as follows:

> The specific objectives of the geological research were (1) to study the stratigraphy and local geomorphology, (2) to distinguish between the natural strata associated with the site and any disturbing and transforming impact of a human presence, identifying the natural and cultural processes of site deposition, and (3) to define those characteristics of the environment that might have attracted humans to the area...
>
> The research was divided into three integrated parts. The first part focused on the interpretation of stratigraphy, petrology, and sediments. The second part, carried out simultaneously with the first, studied the regional context of the site through time and established the different environments present in the late Pleistocene period. This second phase of research compared the Monte Verde data with information collected by other geologists who have worked in the region over the past several decades. And third, experimental studies were carried out for purposes of reconstructing the growth of peat by natural and human agencies (Pino 1989:89).

The Central Valley of the Province of Llanquihue lies between the geologically younger and volcanically active high Andean Cordillera to the east, and the considerably eroded low Coastal Andean Cordillera to the west. Locally, the high Andean Cordillera constitutes a 130 kilometer wide mountain chain with volcanic peaks rising over 2,000 meters above sea level.

> The cordillera is made up of different rock formations in this region, the most important are shales and metagraywackes of the Upper Palaeozoic, Mesozoic clastic sedimentary

> and igneous rocks, and a great variety of Tertiary and Quaternary volcanic, subvolcanic, and pyroclastic rocks. Numerous granitic intrusives cut the Palaeozoic and Mesozoic rock formations.
>
> The coastal range, on the other hand, has a much lower relief; maximum altitudes are not above 950 m a.s.l. It is located almost at the Pacific margin, and made up of metamorphic rocks such as slates, schists, and quartzites of Lower Palaeozoic age (Pino 1989:92).

> The Central Valley is a depression filled by riverine, glacial, and moraine sediments covered by volcanic ash. Monte Verde is located in this geologic unit (Ramirez 1989a:56)

> The Central Valley or Central Depression is found in the graben that separates the two cordilleras. This graben is filled with 2,500 m of alluvial, fluvial, laharic, and glacial sediments (Pino 1989:93).

During the Llanquihue Glacial (equivalent to the Wisconsinan Stage of North America), three major glacial advances (Llanquihue I, II, and III) emanating from the high Andean Cordillera have been identified.

> The oldest of these, Llanquihue I, has an age of at least about 56,000 B.P. The Llanquihue II glacial advance was dated at about 19,500 B.P. The Llanquihue III has an estimated age of 15,000-14,500 B.P.... After 13,000 B.P., all the evidence points toward a rapid deglaciation, interrupted by advances between 4600 and 4200 B.P., around 2500 B.P., and during the past three centuries (Pino 1989:95).

The geology of the Monte Verde site is composed of two geologic formations, the Salto Chico Formation, and the younger Monte Verde Formation. The older Salto Chico Formation was laid down as a graded, cross-stratified broad outwash plain dating to the late glacial Llanquihue II to the interstadial (around 32,000 years B.P.) (Pino 1989:122-123).

> ...it is possible to demonstrate that at the time [Salto Chico] SCH-4 was laid down, the ice was located behind the present position of the Llanquihue II moraine, deposited during the maximum of that advance, at least 21 km to the east of the site. It can also be shown that an extensive outwash was deposited by a large quantity of meltwater from the ice. Evidence for the effects of a periglacial climate such as casts of ice wedges, creeping of strata, or cryoclasticism cannot be found in these outwash sediments nor in any strata of the Monte Verde Formation (Pino 1989:124).

> This unit is made up of clastic sediments, predominantly sand and gravel in varying proportions. The gravel is

> composed of well-rounded fragments of silicic to intermediate plutonic andesites and basalts. The sand, on the other hand, is composed of fragments of the volcanic rocks just mentioned, together with quartz, feldspars, and amphibols (Pino 1989:97).

The Salto Chico Formation is divided into four members (Salto Chico 1, 2, 3, and 4). Locally, at the Monte Verde site, the oldest member (Salto Chico 4) equates to site stratigraphic units designated MV-8 and MV-7 (Pino 1989:98; Dillehay 1988:180). An unconformity separates stratum MV-8 from stratum MV-7 (Dillehay 1988:184). Another unconformity ("erosional discordance") separates the two formations where the Salto Chico member 4 underlies the Monte Verde Formation (Pino 1989:98).

Following the deposition of the four members of the Salto Chico Formation, an erosional event removed Salto Chico members 1, 2, and 3, forming a topographic depression.

> On a smaller scale, this depression is a valley, in contrast to the small gully excavated upstream in Chinchihuapi Creek. Although the depression was formed in the period 31,000-14,000 B.P., its general form has been retained until the present... Irrespective of these dates and events, it is important to note that the surface of SCH-4 must have been stabilized by about 14,000 to 13,000 B.P. when the site was occupied and, as indicated by the presence of the material assemblage in the site, for a long period thereafter (Pino 1989:125).

As indicated by Pino in the previous quote, the cultural remains of Monte Verde II were deposited on top of the eroded sandy surface of the newly exposed Salto Chico member 4. The new deposits (cultural and natural) dating to the site's occupation correspond to site stratigraphic unit MV-6, and to the oldest member of the Monte Verde Formation.

The erosional event (sometime between 31,000 and 14,000 years ago) which locally removed the Salto Chico members 1, 2, and 3, forming the topographic depression or small valley, also permitted the subsequent deposition of the Monte Verde Formation as a localized gallery formation (Pino 1989:101, 127-129). Above the culture-bearing member (MV-6), five other members of the Monte Verde Formation have been identified.

Located directly above the cultural deposits of MV-6, the MV-5 member represents a bog deposit. MV-5 is described by Pino (1989:102) as

> Very plastic, dark yellowish red (5YR3/2) peat comprised of 15 percent to 80 percent organic matter, depending on sample location... This is one of the strata with the least horizontal continuity, generally present only in the site area of the creek basin.
>
> The base of the culturally sterile MV-5 stratum overlying,

> but not encasing, the cultural debris is dated at 11,600 ± 120 B.P. (TX-3472), while the artifacts overlaid by this particular peat layer are several hundred years older. It is important to note that this date does not represent the initial development of the peat. That event is represented by the basal peat lying under, between, and encasing the cultural debris.
>
> Eight dates on cultural materials are available from the surface or shoreline of MV-6 and from the contemporaneous creek bank (SCH-4). These dates range in age from 11,790 ± 200 B.P. (TX-5374) on carbonized wood, through 12,000 ± 250 B.P. (OX-A-105) on bone, to 13,565 ± 250 B.P. (TX-3208) on charcoal from a hearth. The MV-6 dates are interpreted as minimum ages of the formation and principal development of the creek that deposited the gravels (Pino 1989:121-122).

The remaining members, MV-4 through MV-1, are also culturally sterile. MV-4 is composed of medium to fine gravel (Dillehay 1988:180; Pino 1989:101), while MV-3 is composed of a heavily indurated ("cementlike") layer of oxidated iron-rich biogenic precipitates and fine gravels (Pino 1989:107-108).

> Because the MV-3 layer is impermeable, it has also preserved the archaeological materials, acting as a first filter for variations in temperature and precipitation. It also prevented root penetration, animal burrowing, and other contaminations of the cultural deposits (Pino 1989:109).

MV-2 is composed of 25 to 30 cm of fine brownish yellow sand, locally containing thin inclusions of peat. Above this member, the top 8 to 10 cm of dark yellowish brown soil constitutes the MV-1 member (Pino 1989:101).

8.1.4 GENERAL SITE DESCRIPTION

Monte Verde II represents a single stratigraphic unit (MV-6), which has been crosscut by the gently meandering westward flowing modern Chinchihuapi Creek.

> Four distinct zones of buried cultural remains have been located along the ancient banks and terraces of Chinchihuapi Creek... They are: (1) Zone A, on the west side of the north bank of the creek, where the wishbone-shaped architectural structure and patterned concentrations of wood, bone, and stone artifacts were found, (2) Zone B, the south bank of the creek about 300 m downstream from Zone A, where the dental remains of an infant mastodon and two modified pebble flakes were recovered _in situ_, (3) Zone C, on the bank and along a low, sandy terrace of the south side of the creek, where modified pebbles, the remains of a poorly preserved architectural foundation, and scattered charcoal hearths

> were located, and (4) Zone D, on the east side of the north bank of the creek, where the tree branch and limb foundations of agglutinated residential huts and associated activity areas were found in 1983 and 1985 (Dillehay 1988:189).

Dillehay estimates that the main portion of the site sampled in zones A and D covers approximately 800 square meters, while the total area excavated through 1985 in all parts of the site total about 450 square meters (Dillehay 1988:191, 189). Further discussing the overall layout and contents of the main part of the site (in zones A and D), Dillehay (1988:191-192) states

> On the east side of the site [Zone D] the remains of 10 or 11 foundations of residential huts have been recovered. The foundations are formed by small timbers, limbs, and roughly shaped planks usually held in place by stakes. Fallen branches and vertical post stubs reveal that the hut frames were made primarily of luma and other hardwoods. The side walls were placed about 1 m apart against a wall foundation and then apparently draped with animal skins as suggested by the presence of a few small fragments of skin still clinging to the fallen side poles. Results of amino acid tests and microscopic analysis by microbiologists and animal pathologists at the Universidad Austral de Chile and the University of Kentucky suggest that the skins are most likely from a large mammal, probably elephant.
>
> Although the individual house plans differ slightly there are several common features. The hut foundations have the same basic rectangular shape, though they vary in size from approximately 1.3X2.3 m to 4X4.5 m. Entrances into the huts were evidently along an outside wall, probably through hanging hides. A wide variety of plant remains, stone tools, food stains, and small braziers (shallow clay-lined pits for holding burning coals) was found on the living surface inside each hut. The braziers, which contained ash and specks of charcoal, were probably used to heat each hut and warm the food. Cooking was evidently a communal effort, as shown by the discovery of two large clay and charcoal hearths centrally located outside the huts. Flotation studies of brazier and floor contents revealed carbon and edible seeds, nuts, fruits, berries, and tubers. The discovery of three roughly-shaped wooden mortars and several grinding stones near the hearths suggest that the preparation of plant food took place next to the hearths.
>
> The most informative aspect of the architecture on the east side is that these huts were agglutinated and laid out in roughly parallel rows of attached units along the north bank of the creek (see Dillehay 1984, 1986). The presence of lumber piles, limbs, wood shavings, and other modified and partially burned pieces of wood lying close to the hut foundations and hearths show that the occupants stored firewood and made wooden tools. Several pieces of charred and

> partially shaved and carved wooden objects indicate that time was devoted to woodworking. Some of the finished products include three wooden mortars, three hafts on which are mounted stone scrapers... a sharply pointed lance-like implement measuring about 1.5 m long, several digging sticks, and an assortment of other miscellaneous artifacts [references in the original].

Although the site failed to produce any human bones, one of the communal clay braziers preserved the imprint of a child's foot. Additionally, several small pits dug into the ground around one of the large communal hearths contained what appears to be human coprolites (Dillehay 1989:193).

> The west side of the site, which was the main focus of research prior to 1983, is characterized by a nonresidential structure and activity area... The central feature is a roughly ovoid-shaped artificial rise of sand and a few gravels. Resting on this rise is an architectural foundation made of sand and gravel compacted to form a peculiar wishbone shape with a rectangular platform protruding from its exterior base. The total areal extent of this structure measures about 3 m wide, 3.9 m long, and 0.6 m high. Fragments of upright wooden stubs were present approximately every 20 to 30 cm along both arms of the structure. Presumably these are the remains of a pole frame probably draped with hides. Fronting the entrance was a large hearth and a partially preserved rectangular-shaped, wooden, branch-lined plaza. A few stone tools were scattered around the open yard structure. The same type and size of clay-lined braziers recorded on the east side of the site were found on the occupation surface inside the structure and the courtyard. Of particular interest is the association of the hearths and protruding platform with preserved bits of apparent animal hide, of burned seeds and stalks of bulrush reed (Scirpus californicus, a brackishwater plant found today about 30 km west of the site), and of masticated boldo leaves (Peumus boldus, an imported plant found in warmer environments and used by the present-day Mapuche Indians for medicinal purposes).
>
> No other architectural features have been recorded in the immediate vicinity of this structure. There is, however, a semi-circular concentration of hearths, wood piles, wooden artifacts, stone tools, and elephant bones formed around the south and west side of this structure. The majority of the elephant bones, stone tools, made of nonlocal lithic material, and percussion flaked and bifacially worked stone at the site are found in this concentration. The shape, location, and the artifactual content of the wishbone feature suggest that the structure and this end of the site on the north bank of the creek served a special purpose, rather than as living quarters (Dillehay 1988:193-195).

Excavations on the south side of the creek (in Zone C) proved to be disappointing. Poorly preserved wooden limbs were found in a rectangular pattern, largely duplicating the architectural patterns encountered on the north side of the creek (in Zone D). These structures were associated with charcoal scatters and lithic implements suggesting that the south side of the creek had also been occupied. Unfortunately, the banks of the creek at the time of the cultural occupation of Zone C had been somewhat higher in elevation. As a result, the peat which covered and preserved the materials and architectural structures in zones A and D showed only a limited development in Zone C. Test pits excavated higher, along the banks of the south side of the creek revealed only a light scatter of charcoal and a few lithic specimens (Dillehay 1988:193).

Although Dillehay does not specifically make this point clear, the overall pattern of decreasing preservation of perishable artifacts at increasingly higher altitudes on the south side of the creek strongly suggests a gradient of increasingly severe impact by natural transformational processes. If this is indeed the case in Zone C, then this zone is probably one of great interest from an archaeological standpoint.

The meager remains found at the higher elevations of this zone suggest the limited nature of the archaeological evidence which would have characterized the whole of the site if the bog had not formed over the cultural deposits. Furthermore, generalizing beyond this particular site, the gradient in preservation presented by this zone strongly suggests the character of the archaeological record which might be expected from other early sites elsewhere in the Americas, which (at one time) contained numerous perishable remains, and which today are known only from their remaining durable lag of charcoal and lithic artifacts.

8.1.5 TECHNOLOGICAL INDUSTRIES AT MONTE VERDE II

To date, a wide variety of information has been made available about Monte Verde through a series of shorter publications (e.g., Dillehay 1984, 1986, 1987, 1988; Collins and Dillehay 1986; and Dillehay and Collins 1988, 1991), and through an edited volume (Dillehay 1989h). Unfortunately, the vast quantity of perishable artifacts requiring special treatment; combined with the urgency of the field and laboratory tests by specialists in the natural sciences required for an accurate assessment of the status and contextual significance of the site and its contents; and finally, the analysis of the large number of unique and specialized implements recovered from the site, have all conspired to delay the final analysis and publication of the second comprehensive volume aimed at reporting the cultural lithic and non-lithic technology recovered from the Monte Verde site.

Earlier, shorter publications (see above), represent essentially interim progress reports on the status of the recovered data and the analysis of the artifacts. These publications contain only partial data, with some elements which have been superseded by subsequent publications (cf. Dillehay and Collins 1991). Furthermore, although the first comprehensive volume (Dillehay 1989h) contains several articles which provide an overview of the aboriginal technology at Monte Verde, these articles also represent only partial or interim reports. Thus, the discussions which follow (below) can not be considered definitive or final in any sense.

> A rough estimate shows that although lithics constitute approximately 47.4% of the total cultural inventory, they represent only one of four technological industries and one of several categories of assemblages that yield data. When viewed from the perspective of the amount of labor committed to the procurement and preparation of these materials for use, the macrobotanical and wood remains constitute the most important assemblages (Dillehay 1988:189).

The excavation and recovery of perishable artifacts required the use of great care, both to avoid accidental destruction, and to prevent any unintended nicks, cuts, abrasion, or other spurious modifications of the generally soft and waterlogged wooden objects. As Dillehay (1986:321) states,

> A slow and tedious recovery technique, utilizing syringes, horse-hair brushes, soft bamboo splints and water pump and hoses, was required to minimize damage to these remains during excavation. These materials were soaked in appropriate chemicals for conservation and later transported to the archaeological laboratory in Valdivia for further treatment.
>
> In addition to the wooden architectural foundations, several types of artifacts made of wood were recovered, including a sharply pointed lance-like implement, 3 crude wooden mortars, 4 tool hafts or handles, 4 digging sticks, and more than 200 pieces of wood exhibiting cut or planed facets, burned areas, cut marks, and/or smoothed and thinned surfaces (Dillehay 1988:196).

Functional categories can easily be suggested for a number of the wooden artifacts. Such items include the foundation logs and pegs found in situ, the wooden mortars, the digging sticks, the tool hafts (including one directly associated with a stone scraper), the lance with the heat-treated point, etc. Other wooden tools, however, do not immediately suggest the nature of their past functions. One such implement recovered in Zone A, near the wishbone-shaped architectural feature, is described as a "vice-like" implement (approximately one meter in length) with an approximately 10 cm wide carved canal running parallel to the main axis of the implement, and showing a perpendicular cut "bridge" across the canal (Dillehay 1986:328).

Dr. Diaz-Vaz, who conducted careful microscopic and macroscopic examinations of the wooden objects recovered at the site, states that

> Approximately 41 percent of the Monte Verde woods were worked on the edges or points... occurring usually as a cut

> or shavepoint, a cut-snapped edge, or a scraped and blunted tip. The longitudinal sections were modified only to smooth irregular surfaces, flatten a surface by planing, or reduce the thickness of the wood diameter. Most of these modifications were visible to the naked eye (Diaz-Vaz 1989:189).

In addition to wooden technology, Monte Verde also produced a smaller number of modified bone implements. Out of a total of 344 bones recovered from the site, "between 7 and 10 bones were modified as possible digging sticks, gouging tools, or other implements" (Dillehay 1988:196).

> A few tools made of mastodon bones have been excavated. The tip of an unidentified, long bone fragment exhibits small cuts and depressions similar to use marks made on flakers recovered from other late Pleistocene sites in Chile, particularly from the Tagua-Tagua site... Another unidentified bone element [approximately 6.5 cm by 4 cm wide] was formed into a subtriangular body with weak shoulders and a narrow contracting stem... The stem shows specks of bitumen, suggesting that the specimen was probably hafted. Two fragments of mastodon tusk show heavy polish and deeply cut, parallel use marks on their edges. Several other miscellaneous bone fragments evidence cut marks, scorched areas, or modified edges (Dillehay 1989a:16).

As with the wooden implements, some modified bone implements are more difficult to interpret. One such implement is a composite of wood and bone.

> One osteological element which has been identified as a cultural element is an ilium which has a modified branch 'standing' in its center... The bone was recovered from the top of MV-7 and the base of MV-6 about 10 cm below the level which contained other cultural materials. However, the vertically arranged wood protruded up through the strata and thus was located on the same level as the cultural activity area. The bone exhibits numerous shallow, narrow grooves (ca. 3 mm wide) on the surface which holds the wood (Dillehay 1986:332).

Dillehay goes on to note that the wooden branch has been attached to the ilium with bitumen (Ibid).

Beyond these bone specimens showing obvious modifications, Casamiquela and Dillehay examined the entire collection of mastodon bones from the site. On the basis of their observations, Casamiquela and Dillehay (1989:209) point out that

> Although the artifactual data are not presented here, the fact that (1) a significant proportion of the mastodon bone collection [representing a minimum of 7 individuals] is comprised of rib fragments, (2) almost all elements are

> located and patterned in one small area on the heel of a sandy beach and in discrete artifact clusters on the banks of the site (approximately 60 sq m in size) and in association with lithic and wood artifacts and with features, (3) several bones show culturally induced marks, cuts, and fractures, and (4) little evidence for carnivore activity... nor evidence for redeposition, would suggest that human agency is accountable for this collection.

The first attempt to present a systematic preliminary report on the lithic technology at Monte Verde was published by Collins and Dillehay in 1986. This report, however, did not include the results of the objects recovered during the 1983 and 1985 field seasons.

> Three different methods for making stone tools are represented among the specimens from Monte Verde: flaking... pecking-and-grinding and modification through use on some unflaked stones. The flaked and pecked-ground tools resemble artifacts found in the remains of later South American cultures, but the use of naturally fractured stones has not been duplicated among other known assemblages in the Americas (Collins and Dillehay 1986; Dillehay 1984), although single artifacts from some sites are very similar in form to those found at Monte Verde (Dillehay 1988:195) [citations in the original].

The original technical report by Collins and Dillehay (1986), which discussed the lithic assemblage recovered at Monte Verde up though the 1981 field season, included a total of 207 specimens. These specimens were placed in eleven artifact categories including biface (1), chopper (1), core (1), grooved stones (2), spherical stones (14), subspherical stones (22), edge battered stones (3), single-faceted stones (86), multifaceted stones (30), miscellaneous (2), and "other" (45) (Collins and Dillehay 1986:342).

The grooved and the spherical stones each show varying amounts of cultural alterations. The subspherical stones occur naturally, but were selected and/or modified culturally (Collins and Dillehay 1986:347). The single-faceted stones consist of locally occurring small cobble and pebble pieces, each of which has at least one acute edge angle. A significant proportion of these specimens show nicks and dulling along their edges suggesting the effects of use-wear (Ibid.).

The multi-faceted stones represent an elaboration on the pattern presented by the single-faceted stones. Some of the multi-faceted stones are made of materials exotic to the site. Lithological comparisons suggest that they originated along the coast. One of the multifaceted stones made of a crystalline local material is of particular interest.

> One edge of the otherwise hemispherical piece is broken away. The specimen was found seated on a wooden haft with the cleavage plane down and the broken edge protruding

> slightly beyond the edge of the haft (Collins and Dillehay 1986:347-348).

As with the single-faceted stones, several of the multi-faceted stones showed edge modifications characteristic of use-wear, and two other specimens under magnification showed striations on their edges. One of the miscellaneous stones shows striations and a heavy polish on one of its flat surfaces (Collins and Dillehay 1986:348).

Subsequent excavations during the 1983 and 1985 field seasons concentrated on the main village area (Zone D) on the east end of the site, north of the creek. In a recent rebuttal to comments made by Lynch about the Monte Verde site, Dillehay and Collins (1991:337) note that

> The provenienced lithic specimens from the 12,000- to 13,000-year-old horizon now number 680. The 473 specimens not considered by Lynch's review for the most part are consistent in character with the 207 described earlier but include several cultural forms not found or not formerly recognized among the 207. These forms include 2 slender, well-made bifacial points reminiscent of El Jobo points, a ground and polished perforator of basalt, 11 hammerstones, 3 notched pieces, 8 percussion flakes, 30 manos, and 13 thermal spalls. The descriptive categories published earlier (Collins and Dillehay 1986), with the exception of the grooved stones, are expanded with more, and often better, specimens [citation in the original].

In their original preliminary report, Collins and Dillehay (1986:354) concluded their analysis of the lithic assemblage from Monte Verde II by stating that

> Not all the stone objects from Monte Verde may be unquestionably identified as human products. The biface and core are familiar kinds of chipped stone objects, one certainly and the other probably of exotic stone. As no flakes of either material were recovered, it must be inferred that the knapping of these two pieces occurred outside of the sampled area of the site or at other sites. Technologically, direct percussion is indicated by these artifacts. The two nearly spherical, grooved stones clearly are humanly made of local materials, perhaps first being abraded to their spherical form from less spherical natural forms and then grooved.
>
> The hafted split pebble is another established artifact; although in the absence of the haft, its form is little different from many of the objects whose statuses as artifacts are less certain.
>
> At the next level of interpretation are the objects which may be natural but have possible cultural attributes, or were found under conditions strongly suggesting cultural intervention. These include the three edge-battered stones; the two multi-faceted pieces of white quartz; the granitic

> chopper; the split cobble; the spherical stones; and most, if not all, of the subspherical stones. The utility of the wedge-shaped edges on these crystalline rocks has been established experimentally in wood cutting and scraping tasks replicative of those found on the wooden artifacts at the site, and at least 40 of these stone pieces show evidence of use. The original question of whether any of these were artifacts no longer exists; and we are left with determining more precisely which were and which were not actually used.

Such an assemblage, if found in the absence of perishable artifacts and other remains, would likely be overlooked or "shelved" as problematical. Indeed, many of the specimens composing this assemblage might be dismissed if found under other circumstances. A good case in point is the multi-faceted, hafted scraper. Thus, in addition to the multitude of other lessons which one can learn from this site, the lithic assemblage from Monte Verde II holds especially important lessons for those of us working in sites which have considerably poorer preservation of perishable artifacts, and who therefore must define lithic assemblages and reconstruct past activities without the benefit of the wealth of additional data and details presented by a site like Monte Verde II.

8.1.6 CHRONOLOGY AND PALEOENVIRONMENTAL DATA

A total of eighteen samples from various levels of the site were submitted for radiocarbon dating to three separate laboratories. Exactly half of these were obtained from the Monte Verde II (MV-6) cultural stratum (Dillehay and Pino 1989). In order to ascertain the reliability of these samples, each sample was carefully examined and cleaned.

> All radiocarbon samples were pretreated for the removal of uncharred cellulose, including all root materials, humic acid, and fungus. No contaminants were detected by laboratory specialists (Dillehay and Pino 1989:133).

Geologic surveys of the site and its vicinity indicate a total absence of carbonates, coal, or other common sources of potential contaminants.

> These materials do not exist in nearby outcrops of the Salto Chico and Monte Verde formations nor in nearby glacial outwash deposits. Further, there is no evidence to suggest that the samples have been contaminated by foreign coal deposits or particles as a result of alluvial deposition, groundwater transport, or human transport. Several geologists have worked in the region over the past twenty-five years and are acquainted with deep core drilling by the Chilean government and private companies in search of mineral and other resources; they report no coal deposits in the

region (Dillehay and Pino 1989:133).

In addition to the data presented (above), and to the precautions employed by the radiocarbon laboratories, particular care was taken to avoid submitting any cultural samples which might have been contaminated by specks of bitumen or tar pitch (some of which was apparently used in hafting some of the artifacts [see above]) (Ibid.). Finally, geological surveys were employed to determine that the organic materials, including the peat layer which preserved the cultural layer at Monte Verde II (i.e., the Monte Verde Formation members MV-6 and MV-5) represented a local geologic phenomenon which had developed at the site, and therefore did not contain older organic contaminants introduced from elsewhere (Dillehay and Pino 1989:134).

Dillehay and Pino (1989) discuss in great detail the provenience, raw material, sample context, and the results of their radiocarbon samples. When these samples are placed in their stratigraphic contexts, these samples present a concordant sequence of dates with the known geologic and archaeological stratigraphy of the site (see table 6.1 in Dillehay and Pino 1989:134-135).

Focusing specifically on the dates obtained from charcoal and wooden artifacts from the cultural stratum, one finds a relatively tight correlation between all reported dates. These dates include 12,600 ± 140 (Beta-6755); 12,825 ± 150 (OXA-381); 13,030 ± 130 (TX-4437); and 13,120 ± 440 (TX-5375) years ago on wooden artifacts, and 13,965 ± 250 (TX-3208) years ago on charcoal (Dillehay and Pino 1989:135). In evaluating the range of dates obtained from the cultural stratum, and considering the possible introduction of modern contaminants to these samples by water, Dillehay and Pino (1989:140) point out that problems with such contamination would be least likely for the charcoal sample from the cultural stratum (sample TX-3208, above).

> ...the TX-3208 date may best represent the age of the cultural event, because it was sealed and preserved in a clay-lined feature located in a higher and drier bank, about 10 m inland from, and 1 m above, the old creek level, overlaid by the MV-5 peat layer, and was not saturated with modern creek water. If we accept clay and peat as protective sealants, and if we accept minimal contact with the modern creek as an indicator of little contamination, this charcoal date may be more reliable than those obtained on water-soaked materials recovered from areas in closer proximity to the wet creek (Ibid.).

Support for the chronology of the cultural stratum has also been forthcoming from the wide variety of studies conducted by specialists in the natural sciences. The geological studies and stratigraphic correlations derived from the work of Pino (1989), and Dillehay and Pino (1989) have already been discussed earlier. Significantly, the achaeobotanical studies of Ramirez (1989b) and Diaz-Vaz (1989), Heusser's (1989) pollen analysis, Harris' (1989) diatom analysis, Casamiquela and Dillehay's (1989) faunal analysis, and Hoganson et al.'s

(1989) fossil beetle analysis all concur with each other's findings, and with the radiocarbon dates obtained for the cultural materials at Monte Verde.

Based on an analysis of the wood recovered from the site, Diaz-Vaz (1989:191) states that

> The tree species identified suggest that the paleoenvironment during the occupation of the Monte Verde site was probably a temperate, humid one, with shaded and boggy areas near watercourses, an environment and climate very similar to that of today.

Carlos Ramirez, who conducted a much broader survey and analysis of the macrobotanical remains from the site concludes:

> 1. The abundance and preservation of the macroscopic plant remains permit both the general reconstruction of the indigenous landscape and the types of economic resources available to the early inhabitants of the site.
> 2. The list of plant taxa prepared on the basis of macroscopic plant remains from Monte Verde corresponds to, and complements the pollen analysis and the study of the insects, wood, and diatoms found at the site.
> 3. The presence of such different taxa coming from distinct plant formations and from places distant from the site suggests purposive human activity reflected in (a) active gathering of plants, (b) visits to various environments possibly as far away as 200 km or more, and (c) selective exploitation and transportation of usable plant parts.
> 4. Almost all elements recovered from, and identified in, the total plant assemblage have some practical human use, either as food, condiment, construction material, fuel, or as medicine... This does not necessarily imply that some of these were not natural occurrences; it only means that the bulk of the plant remains can be plausibly attributed to human agency.
> 5. Assuming that the majority of these remains were collected for food consumption, we can suggest that the diet was made up primarily of plants.
> 6. The greater part of the food plants identified corresponds to fresh-water and salt-water aquatic species. This suggests an intensive use of both local and nonlocal marshes, swamps, lagunas, and estuaries on the terrace plain and in the floodplain of the Maullin River, and along the coast, respectively. A few plants were evidently obtained from higher elevations in the Andean mountains and from more arid areas farther north, such as Peumus boldus.
> 7. The presence of burned and unburned remains also suggest a controlled and selective use of fire (Ramirez 1989:169-170).

These findings are, once again, in agreement with the paleoenvironmental assessment based on pollen analysis from Monte Verde and nearby locales. According to Heusser (1989:198),

> What amounted to park-tundra earlier than 13,000 years ago, gradually gave way later to arboreal communities. Human occupation at about 13,000 B.P. at Monte Verde was apparently a time of floristic and structural change in the character of the vegetation, which followed the regional trend toward decreasing openness.
>
> Climatic conditions for the interval covered by the pollen record (13,500-11,000 B.P.) are indicated by two sets of curves for temperature and precipitation... The curves, which were generated from two independent sources and show accord for the interval, reveal climate relatively cold and not as wet around 14,000 years, followed by greater precipitation and higher temperature. By 12,000 years ago, conditions were apparently as warm as today, and possibly warmer.

The onset of modern conditions at Monte Verde at the time of the occupation of the site is further supported by several other studies. Harris (1989), who examined the fossil diatoms from the site, comparing them to modern diatoms, concludes by stating that

> The similarities of the modern diatoms further suggests little change in the aquatic environment over the past 13,000 years. Although one would expect stream shifting and subsequent changes in stream beds, the diatom flora strongly supports the idea of similar weather, water quality, and topography. In fact, a glance to the past through the eyes of the ancient inhabitants of Monte Verde would show an ecology and environment similar to what we see today (Harris 1989:204).

These conclusions are also reflected in the comments of Casamiquela and Dillehay (1989) and the comments of Hoganson et al. (1989).

> The Monte Verde faunal assemblage is one of the Pleistocene period. Among the fauna only two animals, mastodon and paleo-lama are extinct. The remaining small animal taxa occur in the area today (Casamiquela and Dillehay 1989:208).

> The similarity between the Monte Verde fossil [beetle] assemblages and samples of the existing fauna of slightly disturbed low elevation Valdivian Rain Forest locations is so striking that it implies that the climate at 13,000 B.P. was similar to that of today. If the climate had been wetter and the mean summer temperatures lower, similar for example, to the climate of the Chilean Channels, the fossil assemblage would have been considerably less diverse (Hoganson et al. 1989:222).

Based on these paleoenvironmental assessments, when considered in conjunction with the site's archaeological data, it seems that the late Pleistocene occupation of Monte Verde II coincides with a period of climatic amelioration and local diversification of plant and animal resources.

> The site inhabitants of 13,000 B.P. were possibly attracted to the upper reaches of the Chinchihuapi Creek for its central access to several different topographic features, particularly to the small depression and stream course with its sandy beaches and banks and its grass-covered terraces, a setting very similar to that of today's... Such a setting would also have provided a well-drained landscape suitable for encampment and immediate access to a fresh-water stream.
>
> Proximity to exploitable food sources must also have been a critical factor in the choice of this location. It provided equidistant access to an ample and diverse supply of inorganic and organic resources (Dillehay 1989f:243).

Significantly, one should also add to these observations that the sheer range of resources utilized at the site, the presence of a well laid out village, and an effective wood, bone, and stone technology all point to an established pattern of cultural existence with a unique local (if not regional) flavor which must have developed from long-standing earlier antecedents. The changes in the local climate which preceded the settlement of Monte Verde probably made the southward extension of the adaptations indicated at the site possible in this area by 13,000 years ago. In this sense, and as a result of the fortuitous preservation of the site and its contents by local conditions, the Monte Verde site represents a unique instant in time, frozen, as in a motion picture still frame, in the long sequence of events which began with the end of the Wisconsinan maximum and proceeded through the Saturation and Crisis periods of the model output, to the rise of the climates and cultures of the early Holocene.

8.1.7 MONTE VERDE II - DISCUSSION AND CONCLUDING REMARKS

As a final comment on the work at Monte Verde we might observe that the sequence of several field seasons, and the early incorporation of a diverse set of specialists from the natural sciences, all supported a systematic and broadly multidisciplinary approach to research. Despite these advantages, the critical aspect of the work at Monte Verde can best be seen as part of the timely application of an appropriate research strategy.

> The Monte Verde project has developed a set of assumptions and a hypothetical model about the life-history and natural formation processes of abiotic and biotic naturefacts and artifacts in the site from the perspective of the Chinchihuapi Creek basin and surrounding environmental setting. (Specific hypotheses about cultural activity in the site, about the artifacts, and about the cultural formation pro-

> cesses will appear in a later volume.) These processes were evaluated by archaeologists and other scientists studying different contexts of past-static and present-dynamic. Excavation in natural and cultural areas in the creek basin and the execution of a series of appropriate experimental, natural history, and folk studies in the basin and adjacent areas were designed to determine whether these assumptions and expectations could be falsified under different conditions. Most of these observations and tests were carried out by an interdisciplinary team studying the recurrent properties believed to reflect natural differences specific to the abiotic and biotic categories under observation.
>
> Each scientist was challenged to test the assumption that there are selected, altered, and patterned materials in the site that are different from those observed in the noncultural and natural-static contexts in the same creek basin and in areas beyond it, and that cannot be readily explained by any known natural process. From this, it was assumed that if nonnatural, that is, cultural, agencies were operating on the Monte Verde site, they should be discernible in both the non-artifactual and artifactual assemblages in the archaeological record...
>
> Each scientist was also asked to identify the processes as results of natural agencies; though, if they perceived patterns and processes to be the result of human agency, then it was their responsibility to explain them as such. Thus, *a corollary aim of the interdisciplinary studies was to test the assumption that humans were the agency most likely responsible for most of the site record under study, although portions of this record may have been deposited or disturbed by various natural agencies through time* (Dillehay 1989b:41) [emphasis in original].

The history and structure of research, Dillehay's own discussions, and the various presentations compiled by Dillehay (1989h) from his interdisciplinary team, all point to the use of an approach dependent upon the formulation and testing of multiple working hypotheses. Furthermore, their results and conclusions, reached by each scientist on the basis of their own independent work, further strengthen the conclusions reached by Dillehay. It is precisely the use of such an archaeological research strategy which has made it possible for Dillehay and Collins to provide a vigorous, authoritative, and conclusive rebuttal to the comments of those critical of their conclusions (cf. Dillehay and Collins 1991).

Obviously every site is different from all others. We may also note that not all sites will be endowed with the rich sources of data which made the use of an interdisciplinary approach at Monte Verde not only possible, but imperative. Furthermore, often other factors unrelated to the evidence presented by a site will alter or mitigate the possibility of using such a thorough and comprehensive approach. Nevertheless, both the data recovered at Monte Verde, and the basic orientation towards the solution of archaeological research problems unequivocably point to the direction in which future work in Proto-American prehistory must be framed if the predictions of the present model are to be reliably tested.

8.2 THE EAST RIM PROJECT - A SECOND CASE STUDY

About twenty years ago I conducted a set of surveys associated with systematic excavation of a site in the Mohave desert. The data recovered pertain to the early prehistory of the Americas, while the goals and methods of research will serve to illustrate another example of the application of some of the principles discussed in this, and the previous chapter.

The comprehensive final report on the work at the East Rim site was submitted as a Master's Thesis in 1975 (Alsoszatai-Petheo 1975). Although excerpts and discussions of the data have been presented in subsequent publications (Alsoszatai-Petheo 1978, 1981; Carter 1980:219-227), the comprehensive final report remains unpublished. The discussion which follows is based principally on the data contained in the final report (Alsoszatai-Petheo 1975), and on the raw field and laboratory data used in compiling the final report. Additionally, reference will be made to subsequent work by myself and others which helps to clarify further the evidence presented by the East Rim site.

The East Rim site is far from ideal, lacking macrofossils, suitable organic materials for radiometric dating, or even stratified multiple components. Nevertheless, the excavation of the site and the related surveys were undertaken precisely as a methodological test case designed to demonstrate that the application of solid archaeological reasoning and methods could be made to yield reliable data even from field conditions which normally result in rejection from serious archaeological consideration. As such, the East Rim site and project represent an ideal case study for discussion at this point.

The inclusion of this site as the second case study in this work is based on several criteria. Having presented a case study of an early site from South America, I wished to include another case study from North America. The Monte Verde site is clearly a late Crisis/Specialization period site. The East Rim site, on the other hand, represents an earlier, Saturation period site. The Monte Verde site is blessed with astonishing preservation of perishable ecofacts and artifacts. The East Rim site, on the other hand, represents the opposite end of the spectrum of preservation of organic remains.

As indicated in the survey of early archaeological sites in the preceding chapter, a large number of early sites were located near riparian or lake shore environments. Detailed discussion of this site permits a closer examination of one such site. Lastly, the East Rim site is also an excellent illustration of the importance of adopting a research strategy, similar to the one adopted at Monte Verde, which relies on the use of an active dialogue between field and laboratory data, and a nested set of multiple working hypotheses.

8.2.1 THE HISTORICAL BACKGROUND OF THE EAST RIM PROJECT

The East Rim site is one of a number of sites initially identified in the 1950s during a series of surveys of the Manix basin (see Simpson 1958, 1960, 1962, and 1975). The Pleistocene Lake Manix basin is located approximately twenty miles east of Barstow, California. The surveys of the basin were sponsored first by the Southwest Museum of Los Angeles and later by the San Bernardino County Museum.

The surveys of the Manix basin led by Simpson in the 1950s were aimed at a general reconnaissance, discovery, and recording of sites in the basin. These surveys led to the discovery of over 100 sites identified from surficial specimens on the basis of artifact typology as characteristic of protohistoric and prehistoric sites of known age and affiliation elsewhere, outside the basin. In her initial reports Simpson (1958:9) noted that there was a sequence which included

> 1—Recent Shoshonean occupation close to the modern lake bed; 2—Above that, on terraces formed as the lake receded, the 4000-8000 year old Playa Complex featuring Pinto Basin-type material; and 3—The entirely different and apparently much older Early Paleolithic-like specimens which may be 20,000 years old or older. In this survey, these artifacts have been found only above the high stand of Pleistocene Lake Manix.

The southern Sierras, the Transverse Ranges, and the Coastal Ranges cast a rain shadow which has shaped the character of the Mohave Desert. In the hot, dry climate of the region, water has traditionally been a significant limiting factor influencing the distribution of plant, animal, and human communities. Springs, largely dry intermittent streams, and ephemeral lakes have played a critical role aboriginally. This role has made them useful geographical and chronological markers in modern archaeological reconstructions (e.g., Campbell and Campbell 1935, 1937). Simpson's sequence of sites and associated shoreline features represents an application of these principles in the Manix basin.

Based on her field observations, Simpson described the occurrence of a series of sites containing an assemblage of apparently simple yet distinctive artifacts belonging to what she named the Manix Lake Lithic Industry. Furthermore, she noted that the Manix Lake Lithic Industry sites were located, without exception, above the high shoreline of Pleistocene Lake Manix.

These observations also led Simpson to propose that the sites were of considerable antiquity, and that the artifacts bore similarities to comparable artifacts from the Paleolithic of the Old World. These last claims attracted the attention of American archaeologists working with the early prehistoric sequence of the New World. Krieger (1964) included the Manix Lake Lithic Industry in his "pre-projectile point horizon," as did other subsequent authors (e.g., Willey 1966).

By the late 1960s these sites had generated a considerable amount of speculation and disagreement, yet still no new data were available beyond Simpson's early survey reports. Controversy over the significance of these sites, coupled with a lack of detailed and definitive data obtained from controlled excavations, encouraged casual visitors to espouse their views on the basis of "first hand observations." This was the context within which the East Rim project was conceived.

Leakey and Simpson's on-going excavation of the Calico Site (SBCM 1500), under the explicit expectation of finding buried Manix Lake Lithic Industry specimens in stratified contexts, added further confusion to the debate about the Manix Lake Lithic Industry sites (e.g., Irwin 1971:45). Evaluations of the character of the buried specimens from SBCM 1500 were mistakenly extended to the surficial and near-surficial Manix Lake Lithic Industry materials despite the presence of a thick sterile layer separating them at SBCM 1500, and despite Simpson's resultant conclusion that "there is a massive time gap between the two occupation elements" (Simpson 1975:240).

My own interest in the Manix Lake Lithic Industry dates back twenty years to this period. The frequently-encountered claims indicating that little or nothing could be gained by excavation of open surficial desert sites beyond what was already known from "walk-through" surveys and collections needed to be tested in view of the rapid increase in adverse impact upon such sites by modern activities.

Additionally, it seemed clear that the confusion engendered by the debate about the nature, chronology, and significance of the Manix Lake Lithic Industry could be alleviated by testing a variety of hypotheses through additional surveys and controlled excavations at one such site. This dual set of goals, then, represent the primary aims of the East Rim project. The specific methodological goals of the project are not as significant for the present discussion as are the hypotheses, data, and conclusions concerning the Manix Lake Lithic Industry.

8.2.2 DISCUSSION OF HYPOTHESES AND COROLLARIES

Based on the earlier surveys and publications by Simpson (1958, 1960, 1962) and the general nature of the debate concerning these sites, it appeared that there were four possible hypotheses which might explain the data presented by Manix Lake Lithic Industry sites. These sites might represent:

1) Collections of "geofacts" or "naturefacts" representing the result of natural flaking unrelated to human activities.

2) Mixed activity sites representing Holocene activities by known prehistoric groups.

3) Specialized quarry-workshop sites representing Holocene activities by known prehistoric groups.

4) Sites presenting evidence of various undetermined activities by as yet unknown, earlier populations.

It was further decided that each of these hypotheses might be identified with a series of related consequences as follows:

1) If the implements of the Manix Lake Lithic Industry were geofacts/naturefacts, the most likely mechanisms would include water transport and flaking, or ventifaction produced through wind abrasion. Sand blasting produces faceting (ventifaction) which only superficial-

ly resembles flaking. If sand blasting were the sole mechanism, technical traits such as platforms, platform preparation, bulbs of percussions, and other such characteristics associated with bona fide flintknapping would be absent. Furthermore, microscopic studies of the associated quartz grains should reveal a characteristic high incidence of dull or frosted grains resulting from wind transport; a rounded and glossy or polished appearance resulting from water transport; or a high incidence of angular grains of rough appearance when there has been little or no transport by wind or water.

Water transport would most likely result in a sorting process of gravel-size particles so that light pieces with proportionately large surface areas would be transported more easily, and therefore found separated from parent cores (cores being proportionately heavier relative to their surface area). Furthermore, water transport would result in decreased angularity or increased rounding. Water transport would also result in the abrasion of the surfaces of flakes and cores, producing less clearly defined borders for negative flakes, and dulled flake edges.

Another critical difference which separates natural flaking processes from human flaking processes is the relative likelihood of producing flakes and the likelihood that a single specimen will show more than one incidence of flaking. Each situation is unique in its own ways, and there are no absolute standard values which can be applied and defended. Nonetheless the logic which separates them can be detailed as follows.

The vast majority of stone tools known either from prehistoric sites world-wide, or from ethnographic situations, or from replicative studies have been shaped by the systematic removal of multiple flakes designed to shape an object. Such objects (separate from functional interpretations) present a series of non-random attribute clusters which recur in multiple specimens within sites and between sites.

By contrast, natural flaking processes overwhelmingly tend to be random processes. Whatever the responsible natural agency, the likelihood that any suitable piece of stone should be struck simultaneously with sufficient force, at the correct angle, and at the correct spot to dislodge a flake can be viewed as the product of the likelihood of each of these possibilities. Few situations in nature fulfill all these requirements simultaneously. Where they are met, however, it is certainly possible to find naturally-flaked stone.

What is significant, however, is that even in these situations other clues will be present. As noted earlier, water transport will yield specific and related clues. In other instances, such as natural fracturing due to thermal shock or exfoliation, the typical cluster of technical traits which characterize flintknapping will be absent, with only a low frequency of individual specimens likely to show an occasional shape which mimics such traits. Unlike specimens resulting from human flintknapping, the angles produced by such natural processes will include a high incidence of flaked edges which exceed ninety degrees (cf. Barnes 1939). All of these clues can be used to contrast natural from intentional flaking processes. The most important difference, however, relates to the low probability of natural flaking processes.

Alluvial fans present a good example of a naturally-occurring environment within which unsorted rock particles of different size accumulate and press against each other under the action of gravity. Slides and mudflows can further increase the chances of natural flaking by the introduction of rapid movement. Alluvial fans containing rock material amenable to flaking are therefore likely to contain naturally-flaked stone. This fact, at least, is not at issue. What is at issue is the frequency of such natural flaking events.

The frequency of such events will be the product of the chances that each of the component requirements is met. Thus to produce a flake through natural processes we must have a] flakeable stone positioned within such a matrix so that b] pressure or a glancing blow will be delivered to it from another stone in the matrix c] with sufficient force [either statically or as a blow] d] at the appropriate portion of the rock, and e] at the correct angle to dislodge a flake. How can these requirements be translated into practical terms?

Obviously a matrix which lacks suitable stone for flaking will not contain flaked specimens. However, if we assume for purposes of discussion that an average given cubic measure of matrix contains 20,000 rock particles of which five percent are also of material suitable for flaking, we cannot immediately assume that there should be 1000 flaked specimens within this cubic measure. Other requirements must also be met. At best, this single observation can tell us that there can potentially be up to 1000 naturally flaked specimens out of the total of 20,000 rock particles in the sample.

In order to flake a stone, energy must be transferred differentially with a high gradient of difference within a small distance. In normal flintknapping this principle means that wherever pressure or force from a blow is transferred to a core, that force is concentrated at a small area and in a specific direction when compared to the rest of the core. This action produced a shearing motion resulting in a flake. The presence of such a concentrated and differential force gradient is an absolute requirement for the production of flakes.

Gravity and the resultant accumulated weight of sediments can exert enormous force—well in excess of any force which flintknappers can exert. Additionally, the strength and direction of the force of gravity acting on particles in natural sediments tend to be relatively uniform. Concentrations [steep gradients] of force within small areas required by flaking processes are highly unlikely. Furthermore, as particle sizes decrease, they will increasingly tend to deflect such force randomly, reducing its concentration. Thus the likelihood of transferring a steep gradient of force to a rock will decrease as the proportion of sand and other finer materials in the matrix increase. This relationship can be expressed, in part, as the ratio of rocks within the sample to the number of rocks in actual direct contact and as a function of the force available for flaking which increases with cumulative weight at greater depths. Thus the likelihood of natural flaking will vary in direct proportion to depth and in inverse proportion to the proportional volume of the total sample represented by finer components in the matrix.

In our hypothetical example, if there were an unsorted mixture of particle sizes within the sample which results in an average ratio of direct contact between rocks of one in twenty, then (in our hypothetical sample) the chance that a stone of suitable material might come in direct contact with another stone is one in 400, or 50 out of 20,000. This analysis still fails to address the issue of sufficient force, direction of force, and the location (on the core) of the transfer of force.

When these additional factors are considered in conjunction with the two factors calculated thus far, it becomes obvious that the chance of natural flaking per volume of unsorted rocks and matrix in an alluvial fan is very low, indeed. Assuming for argument's sake a one in ten chance for each of the other three requirements (amount, relative direction, and location of force), the chances of natural flaking are reduced to 1 in 400,000. In our hypothetical cubic volume this analysis would yield a five percent probability of producing one single flake.

On the other hand, this analysis should not be misconstrued to mean that natural flaking is impossible; or that naturally flaked materials could not be concentrated by such other natural processes as flowing water, given the right set of circumstances. Of course, if this were the case, the action of such additional natural processes would have to be demonstrated separately.

Finally, it must be remembered that each flake in our example had a one in 400,000 chance of being produced. Thus, each subsequent flake also has the same odds of being produced. Therefore, a single stone with two naturally produced flakes has a one in 160,000,000,000 chance of being produced, while one with three flakes has a one in 64,000,000,000,000,000 chance of being produced. These are very low odds which make such events highly unlikely but not impossible.

On the other hand, the chances that nature might produce an assemblage of flaked stone specimens with multiple flaking on individual specimens within a small area of an old surface or a narrow stratigraphic zone, and containing specimen types defined by recurrent attribute clusters in non-random spatial distribution by specimen type, is so remote as to preclude serious consideration. Thus, beyond the requirements of flaking and their associated probabilities, it is ultimately the incidence of higher levels of patterning at archaeological sites which allows us to reject natural agencies when evaluating the origin of the data presented by sites.

Returning to the four hypotheses and their associated requirements, it becomes obvious that allocating the materials found at the East Rim site to the work of known Holocene prehistoric groups from the area would very likely carry another set of related consequences.

2) If the remains from the East Rim site represented the (undetermined) activities of known Holocene groups, then we should expect similarities between the materials recovered at the East Rim site and other prehistoric assemblages in the area on the basis of a) artifact types, b) choice of raw materials, c) proportion of artifact types in the assemblage, d) spatial location relative to topography and relevant coeval resources, and e) comparable relative age as determined by various observations such as surface weathering and patination of surficial materials or other more direct determinations. Specialization of activities might well negate some of these criteria, but it is

unlikely that none of these criteria would be met if the East Rim assemblage truly represented the remains of a previously known Holocene group's activities. To some extent, these same predictions should apply to the third hypothesis.

3) If the assemblage from the East Rim site represents the remains of quarry/workshop activity by some known Holocene group, we should find a collection of implement categories and proportions typical of known Holocene quarry/workshop sites. The spatial distribution of these materials would indicate quarrying and workshop extraction activities (not maintenance activities), with a low incidence (though not a complete absence) of finished forms. Finished forms could be defined on the basis of secondary and tertiary flaking, edge-retouch, and edge wear. Occasional finished pieces would also conform by material and style to artifact types from other regional Holocene sites.

4) Failing to demonstrate the applicability of the first three hypotheses to the East Rim site, we would be justified in pursuing and exploring the possibilities presented by the fourth hypothesis. As with any site presenting completely new data, it would be our responsibility to observe and record any and all data which might bear upon the age of the site, the physical characteristics of the assemblage and the site, its spatial dimensions, and to conduct tests designed to reveal the nature of the prehistoric activities which took place at the site.

8.2.3 RESULTS OF INDUCTIVE TESTS FOR THE EAST RIM PROJECT

The East Rim project included both excavation and surveys followed by laboratory analysis and specialized tests. The final report (Alsoszatai-Petheo 1975) details information from all of these sources, systematic comparisons to other sites, and over a dozen appendices containing the results of numerous specialized ancillary investigations. No attempt will be made here to discuss in detail the methods and data obtained from these studies. Instead, the primary goal will be to address specifically those issues which have been raised by the preceding discussions of hypotheses and their corollaries.

The first hypothesis stated that the East Rim site represented "collections of 'geofacts' or 'naturefacts' representing the result of natural flaking unrelated to human activities." In order to test this hypothesis we must present the geographic and geologic context of the site.

The East Rim site is located atop one of several east-west trending ridges at the eastern edge of a low eroding alluvial fan, the Yermo alluvial fan. There are no immediate or directly connecting areas of significantly higher ground near the site although the main axis of the ridge slopes from the west (across the site) toward the east at an average rate of three centimeters per linear meter. To the north and to the south, perpendicular to the main axis of the ridge, the ground slopes downward toward a sequence of modern dry washes. The central axis of the site (paralleling the top of the ridge) covers over 300 square meters and has little appreciable slope, while the overall north-south cross section of the ridge exhibits a slope of about ten centimeters per linear meter.

As a result of these topographic conditions any movement of materials would be slow and directed away from the site—while the gentle slope toward the west is clearly insufficient to allow for significant motion of particles. Similarly, slope wash is not likely to have played a part in moving particles onto or through the site under these conditions.

Excavation and trenching at the site revealed a stratigraphic cross section composed of three primary strata. The topmost stratum consisted of a well-knit desert pavement containing unsorted rocks, gravel, and artifacts of varying sizes. Non-artifactual particles sorted by size show a normal distribution curve with a mean size around eight to nine centimeters.

The second stratum consisted of a yellowish fine sandy loam, grading to a coarse sandy loam at the bottom of the stratum. The matrix composing this stratum extended as a thin lens shape variably ranging in thickness from a maximum of around 20 centimeters along the central east-west axis of the site and gradually disappearing altogether with increased slope toward the north and south edges of the site. This second stratum also contained some gravel and slightly more than forty percent of the archaeological specimens recovered from the site.

The upper part of the third stratum was composed of an increasingly coarser sandy loam grading into an indurated carbonate-rich zone filled with increasing quantities of unsorted rocks and gravel. This third stratum was devoid of artifacts.

In light of this stratigraphic sequence, coupled with the topographic characteristics of the site, we must conclude that these observations are inconsistent with an hypothesis which holds that natural mechanical movements in the matrix were responsible for the production of the assemblage from the East Rim site. Furthermore, the East Rim assemblage contained flaked specimens, identifiable as cores and flakes, unsorted by size. In several instances, cores were found surrounded by flakes removed from them. In a number of instances, flakes were reassembled onto their parent cores. The distance from the flakes and the parent core typically averaged less than one meter, and in some cases ninety percent or more of the original core could be reconstructed by replacing the flakes.

The reconstruction of cores indicates a sequence of repeated flaking of a single nodule of raw material. Flakes were not removed randomly, or merely sequentially struck from one direction. The cores appear to have been flaked in a sequence consistent with decision sequences characteristic of modern experimental replication of human flintknapping. The nearness of component flakes indicates that the highly energetic process responsible for flaking was clearly focused to the extent that it did not alter or scatter vertically or horizontally the resultant flakes over wide areas. Finally, there appears to have been no sorting of particles either by size or by the relative ratio of mass to surface area among flakes and debitage. None of these observations are consistent with random natural flaking processes.

The assemblage from the East Rim site contained numerous specimens which not only showed multiple flaking but whose overall shape and characteristics were directly due to flaking over the entire surface of the specimen. Such specimens showed a sequence of flaking which by comparison to modern flintknapping techniques would have required initial shaping by hard hammer percussion, followed by soft hammer percussion; and the specimens often exhibited controlled retouch along their edges. Such specimens constitute the vast majority of the specimens in the assemblage which were classified as "finished artifacts."

Approximately three hundred such specimens were recovered. The specimens showed a range of sizes and types of flaking. Both unifacially and bifacially flaked specimens were well represented. All these specimens showed consistent attribute clusters which led to their classification into slightly over a dozen consistent artifactual categories—each containing multiple examples. Once again, there is no known natural mechanism which could produce such effects, especially over such a limited area.

Studies of the degree of angularity or relative rounding of the unflaked gravel in the matrix containing the specimens, together with a microscopic study of the micromorphology of the quartz grains in the surrounding loam are also significant. Using standard 100 specimen samples, the unflaked gravel was consistently closer to subrounded than to angular—indicating a significant degree of shaping by water transport. Presumably this water transport occurred as part of the process which deposited the gravel in its present location.

The action of water was also evident in the micromorphology of quartz grains. Standard samples of 100 quartz grains were found to contain between 28 to 44 percent rounded grains, corroborating the influence of water transport seen from gravel analysis. By contrast, flaked specimens lacked any indication of rounding. Dulled edges of flaked specimens occurred in consistent association with tertiary flaking and retouch. Such edges were typically restricted selectively to a portion of the edges of individual specimens, with other portions remaining sharp.

In a number of specimens, dull retouched edges did not protrude, but instead were confined to notched or concave portions of the edge while projecting edges remained considerably sharper. Finally, a number of the specimens showing retouched, dull edges also showed a significant degree of microspalling and striation patterns consistent with similar edge alterations found on modern experimental specimens which had been used as tools for processing softer materials. These contrasts between the surrounding natural gravel and quartz grains and the flaked specimens clearly points to consistent and significant differences in the genesis of these two classes of lithic materials.

Although at the time of the original excavation and analysis of the materials from the East Rim site no attempt was made to apply the "Barnes test" (Barnes 1939) to the assemblage, the original laboratory analysis included a provision for the measurement and recording of the edge angles of all recovered artifacts. Unfortunately, edge angles were recorded by standard intervals of ten degrees rather than as raw numbers. Still, these data can be used to derive comparisons with other assemblages. On the basis of these records, it is possible to characterize the assemblage from the East Rim site as follows:

Less than one percent of the East Rim assemblage exhibited edge angles greater than 80 degrees or less than 20 degrees. The first quartile value falls between 40 and 50 degrees. The second quartile falls between 50 and 60 degrees. The third quartile falls between 60 and 70 degrees. Over 75 percent of the artifacts show edge angles between 40 and 70 degrees. Taken as a whole, these observations fall squarely within the range of edge angle measurements characteristic of assemblages produced through human workmanship and outside the range of assemblages of naturally fractured materials as defined by Barnes (1939). Thus, once again, we are forced to reject the first of the four hypotheses. The East Rim site's assemblage is clearly the result of human activity—not the result of natural (non-human) processes.

The next two hypotheses predicted that the assemblage from the East Rim site represented either a "mixed activity site representing Holocene activities by known prehistoric groups"; or a "specialized quarry-workshop site representing Holocene activities by known prehistoric groups." These two hypotheses each represent variant alternatives which require the East Rim assemblage to be the result of some type of activities by a Holocene prehistoric group or groups.

Archaeological work in the region had been reported by Rogers (1929, 1939), Campbell and Campbell (1935, 1937), Amsden (1935, 1937), Smith et al. (1957), Simpson (1958, 1960, 1962, 1965), Pourade (1966), Davis et al., (1969), Glennan (1974), Bettinger and Taylor (1974), and others. These reports had provided a series of regional frameworks and data about excavated sites against which the East Rim assemblage could be compared.

One of the active components of the East Rim project involved visiting sites, examining archaeological collections in museums, and conducting (where possible) related and follow-up interviews with the responsible archaeologists. First-hand examination of museum collections and in-depth interviews with archaeologists spanned both the local area and portions of three other states plus Baja California. Beyond the data obtained through surveys of the literature, regional sites of different ages with a wide variety of archaeological evidence were evaluated first hand.

On the basis of this work a number of sites bearing the closest resemblance to the East Rim site were formally discussed in the final report. Although many archaeologists provided informal comments and input, formal documentation was limited to several statements and interviews documented at the end of the (1975) site report. The comparisons and assessment of similarities and differences between the data from the East Rim site and other known sites was based collectively on all these sources.

The results of these comparisons must be expressed in the context of specific levels. Comparisons were made to 1) broad synthetic categories ("stages" and "traditions"), 2) to individual sites, and 3) to artifacts from other sites.

As noted earlier, despite the widespread discussion of the Manix Lake Lithic Industry in the literature at the time of the planning, excavation, analysis, and the preparation of the final report on the

East Rim site and project, there was no published information based on any comparably controlled excavation of a Manix Lake Lithic Industry site. Excavations at SBCM 1500 (the Calico site) were aimed at recovering information from deeply buried strata. Excavations at this site were on-going, with most of the attention of both visitors and excavators focused on the deeper deposits. No site report or preliminary manuscript detailing the surface and near-surface finds (containing purported Manix Lake Lithic Industry materials) was available. Even to this day, the East Rim site remains the only systematically excavated and formally reported site of its kind wherein excavations and analysis were approached with the presumption that the character and age of the site were unknown and open to testing.

During the time of the original East Rim project, the principal source of information consisted of a series of brief publications by Simpson (1958, 1960, 1962) exclusively derived from first hand surveys which were the basis for definition of the Manix Lake Lithic Industry. Although there is no doubt that the East Rim site represents one of many sites in the area which fit Simpson's criteria for inclusion as a Manix Lake Lithic Industry site (see Simpson 1975), it is less clear whether the East Rim site represents a typical, marginal, or an atypical example. Furthermore, based as they were on observations derived from surveys rather than excavations, it is difficult to place stringent limits on the precise criteria which might be applied to test whether or not a given site belonged to the Manix Lake Lithic Industry. Lacking other systematically excavated and reported sites, there is no basis by which definitive comparisons can be drawn, even today.

The works of Rogers (1939) and Davis et al. (1969) represent pioneering attempts providing broad regional syntheses. The first of these was focused on the Colorado Desert region to the south, while the other was based on intensive work to the north in the Panamint valley. Pourade's (1966) work extends Roger's earlier synthesis to a much broader region including the Panamint valley region. An important underlying message of all three works relates to a persistence of basic artifactual styles and adaptive strategies, both of which were gradually changing through a series of stages over thousands of years.

Published reports, sites, and museum collections were examined in relation to the data and specimens collected at the East Rim site. The final report indicates that none of these sources produced evidence for any other single site in this region which could be directly equated with the East Rim site. At the same time, several other observations could be made.

1) The artifacts recovered at the East Rim site are clearly reminiscent of the artifacts attributed to the earliest stages in both the Colorado Desert and Panamint cultural sequences. These similarities decrease through time. Thus, the East Rim site fits the general regional pattern without producing a clear-cut match to any specific site or any previously defined stage.

2) One possible similarity between the East Rim site and other known sites relates to Feature One. This feature, located atop the ridge, consisted of an oval depression measuring approximately six by ten meters, gently sloping towards a central point 20 centimeters below the level of the surrounding surface.

The feature formed a clearing covered by well-knit desert pavement. Over eighty percent of the unaltered gravel composing this pavement consisted of particles smaller than three centimeters in length. None of the gravel particles exceeded ten centimeters in length. By contrast, large numbers of rocks measuring well over ten centimeters (including some measuring forty to fifty centimeters in size) were found in concentrated piles within two or three meters of this feature. Statistical counts of rocks from the carbonate-rich third stratum below this feature and from the surface elsewhere at the site, produced a normal distribution curve which markedly contrasted to the skewed particle size distribution on the surface in Feature One.

As with other more specific traits presented by the East Rim assemblage, Feature One presents a passing resemblance to the "house" or "sleeping circles" associated with early sites in Roger's (1939) Malpais/San Dieguito sequence. The resemblances, however, appear to be superficial. Feature One at the East Rim site differs from true "sleeping circles" in that its surface was covered by well-knit desert pavement; in that it lacked an organized ring of larger rocks outlining its perimeter (this trait, though common, is not found universally elsewhere); and in that it contained within this area (representing a little over ten percent of the site's area) fully one third of all the finished tools recovered from the site—representing every major artifactual category but lacking any primary or large secondary flakes. To my knowledge, all known "sleeping circles" (as described by Rogers and others) differ in one or more of these traits from Feature One at the East Rim site. Furthermore, locational analysis of the recovered artifactual material from this feature (unlike elsewhere at the site) failed to yield a consistent pattern of association of artifactual categories.

3) The location of the East Rim site relative to modern and extinct water sources is also congruent with the definition of the earlier stages of Rogers' Malpais/San Dieguito and Davis' Western Lithic Co-tradition. The East Rim site (and other Manix Lake Lithic Industry sites) are located atop an old eroding surface overlooking the Manix basin. Related sites containing an equivalent assemblage of artifacts are found both at and near the high shoreline of extinct Pleistocene Lake Manix, but not below this level.

The Yermo alluvial fan covers several square miles, forming a tongue of land which must have projected into Lake Manix at the time that the lake crested. Thus, not all the sites of this type are located on the shore of the lake; yet the lake would have been easily accessible in three directions from nearly anywhere on the fan. Interestingly, an isolated hill to the southeast of the Yermo fan (Harvard Hill) would have occasionally formed a near-shore island in Lake Manix. Surveys of this hill also point to the presence of artifacts equivalent to those found on the Yermo fan, but again only above the high shoreline mark.

There are no known modern or historic springs anywhere near the East Rim site. This conclusion is based on a survey of maps, historic literature, and several first-hand attempts at aerial reconnaissance including aerial infrared photographic surveys conducted near the site (at least once following an episode of heavy rainfall) in an attempt to discover previously unknown seeps or other sources of water.

Known modern springs are located at the foot of the mountains across the basin, with only one spring located as close as five miles from the site. Whereas Holocene sites are often found along the edges of ephemeral lakes in the valley bottoms and near active springs (cf. Hall 1976), the East Rim site and other Manix Lake Lithic Industry sites do not fit this profile.

Overall, these observations compare favorably to the descriptions of the geographic/geomorphic contexts of sites assigned to the early stages of the Malpais/San Dieguito and Western Lithic Co-tradition, but clearly contrast with the contexts of later Holocene sites. Early sites in this region are typically found on locally elevated remnants of old surfaces close to or surrounded by more recently eroded areas—usually near extinct lakes or old drainage channels (see Rogers 1939:27; Pourade 1966:39; Davis et al., 1969; and King and Casebier 1976:22).

4) There are no clearly defined and consistent typological criteria which could be used to correlate the East Rim site decisively with any of the early sites on the basis of single tool typology. Typological similarities exist only in broad categories including artifactual categories (as opposed to diagnostic types), flaking techniques, and other technological attributes derived from a diachronic regional perspective.

5) The East Rim project yielded a single projectile point as an isolated surface find more than forty meters to the east-southeast of the site. Comparisons of raw material, flaking technique, and the absence of surface weathering of this specimen all contrast with the assemblage from the site. Comparisons with projectile point types in the region suggest that the specimen can best be compared to straight-based Cottonwood Triangular points dating to the last millennium, and possibly to the last 500 years (see Warren 1985:375; and Waugh 1988:112). On this basis the specimen was not considered to be coeval with the remainder of the evidence from the East Rim site.

6) Although many generalized artifactual categories at the East Rim site compare favorably with individual specimens and portions of the assemblages at other early sites in the region, the complete assemblage at the East Rim site contains both a greater diversity of tool types and a larger proportion of smaller and retouched tools than is normally characteristic of early sites in the region. Many specimens appear to have been fashioned on smaller flakes and small core pieces, showing delicate retouch only occasionally seen on larger specimens. To some extent this appears to be a unique feature separating the East Rim site from other early sites in the region. It is unclear, however, to what extent these differences are the result of genuine differences in the original assemblages or merely the result of the differences in the composition of assemblages collected as part of surface surveys as opposed to careful excavation and screening.

7) Although differences in the methods of collection may have obscured potential comparisons in the composition of the artifactual assemblages, the systematic excavation and recording of recovered materials from the East Rim site permitted the subsequent analysis of

artifactual categories according to their distributions within the site. This analysis provided data on associations and artifact groupings or clusters which suggest task-related use and discrete activity areas within the site. Such data (discussed in greater detail below) are largely unavailable from other early sites, precluding direct comparisons.

Although the East Rim site offers some general similarities to early sites in the Malpais/San Dieguito sequence and to early Western Lithic Co-tradition sites, many of the traits of the assemblage and other data are sufficiently different to set this site apart conclusively. On the basis of detailed comparisons it is impossible to classify the East Rim site as clearly belonging to any specific known prehistoric phase identified in the region. Thus the criteria of the second hypothesis have not been met fully—though general resemblances to other early sites in the region can not and should not be dismissed without future analysis of the data presented by other sites.

The third hypothesis is closely tied to the second hypothesis (as noted earlier). Although the East Rim site does not correspond to any specific known site or stage previously known in the region, this third hypothesis has also been weakened somewhat. However, the East Rim site might be better correlated with other known assemblages if it represents a specialized extraction site. This last conclusion could only be tested through the application of standard methods of excavation and data analysis typically employed at stratified sites. Had the work at the East Rim site been limited to only controlled field survey techniques, as is often the case at other presumed and actual quarry/workshop sites in desert areas, the conclusion that the East Rim site was simply another such site could not have been adequately tested.

It should be noted that the final report on the East Rim site stated very clearly that the lithic remains represent refuse from lithic workshops but not from quarry activity. The site yielded about five thousand flakes and pieces of debitage from 800 square meters (i.e., slightly over six lithic fragments per square meter); many of these flakes represent small tertiary trimming flakes rather than primary or secondary flakes. No picks or mauls were recovered.

The excavation techniques employed at the site included screening through 1/4 and 1/8 inch mesh screens. Undoubtedly this approach is partly responsible for the recovery of not only a large number of primary and secondary flakes, but also of numerous tertiary flakes and small pieces of debitage indicative of both general lithic workshop activities and other tool finishing or resharpening activities.

The application of careful excavation and recording techniques coupled with careful locational analysis permitted the identification of discrete foci of activity which yielded a much more detailed picture of the prehistoric activities at the site than would otherwise have been possible. As a result of these controls and subsequent analyses, it is clear that various different activities took place at the site.

Prehistoric activities at the site resulted in the production of unmodified cores and large primary flakes and debitage from locally occurring chalcedony and jasper. A number of artifacts representing unfinished blanks and preforms were also recovered, but a major portion of the lithic remains included secondary and tertiary flakes. These flakes together with numerous well-formed artifact types indicate the production of finished functional tool types at the site.

In her initial reports of the Manix Lake Lithic Industry sites, Simpson (1958, 1960) focused upon the occurrence of large tool types including large core tools which she compared to Old World Lower Paleolithic specimens. In contrast, the collection from the East Rim site yielded a preponderance of flake tools, many of which were relatively small (between 10 and 2 or 3 centimeters in size) and frequently showed careful shaping through tertiary flaking and edge retouch. Other small tools included carefully shaped and retouched small core pieces. Of the total assemblage at the East Rim site, the twenty-four larger ovate bifaces recovered represented only about eight percent of the shaped artifacts.

Microscopic examination of a nested random sample (N = 40) of specimens by distinctive categories yielded evidence of microspalling, wear-related striations, or both from two-thirds of the categories. General workshop flakes, cores, and blanks failed to produce evidence of microspalling or striations. Sixty percent of the other artifactual categories showed evidence of microspalling, while ten percent also showed microscopic use-wear striations.

Finally, a number of artifacts (other than cores, blanks, and workshop flakes) showed a sequence of flaking and retouch (mostly on smaller specimens but occasionally on larger specimens as well) which were further modified along the edges by the occurrence of minute step flakes or a smooth blunting of the edges. Both of these effects were experimentally reproduced in the laboratory when modern copies made of equivalent material were experimentally used to work wood and other softer materials.

The repeated co-occurrence of associated tool types forming discrete foci of activity away from locales of primary workshop activity (defined by the presence of flake concentrations surrounding parent cores), coupled with the common occurrence of what may best be interpreted as finished, used tool types at the East Rim site, led me to conclude that both lithic procurement and other maintenance activities took place at the site. Feature One tends to corroborate this conclusion.

Feature One contained an unusually high concentration of artifacts relative to the overall frequency of artifacts elsewhere at the site. Finished and used artifacts were common among the specimens found in Feature One. Despite these observations, no discernible association pattern emerged from an analysis of the location of the artifacts found within this feature. Furthermore, with the exception of proportionally few, small, retouch and tertiary flakes, Feature One produced no evidence of lithic extraction activities. Given these observations in conjunction with the physical details of the feature described above, the best interpretation of this feature led me to conclude that it formed the focus of household habitation and maintenance activities.

In light of these data and interpretations, it is highly unlikely that the data presented by the East Rim site could be the result of intermittent and casual lithic procurement activities. Whereas lithic procurement activities certainly took place at this site—even to the point where it might be argued that the location of the site is significantly tied to the availability of raw materials for lithic procurement activities—the remainder of the evidence essentially negates the validity of the third hypothesis. The East Rim site is not simply or exclusively a specialized quarry/workshop site resulting from the activity of some known and previously described group. Given this conclusion, we must now turn our attention to the fourth hypothesis.

Among the principal issues raised by the rejection of the first three hypotheses was the need to establish the age of the East Rim site. In as much as the East Rim site appeared to be an example of a Manix Lake Lithic Industry site (see Simpson 1975), the relationship between these sites and modern and extinct bodies and sources of water became a prime consideration. Simpson's (1958, 1960) early reports indicated that these sites had been found only above the high shorelines of extinct Pleistocene Manix Lake, and never below this level. The surveys conducted as part of the East Rim project failed to falsify this observation.

Radiocarbon dates on organic tufa (Fergusson and Libby 1962:113; Hubbs et al. 1962:227), together with geomorphological analysis of land features in the area of Afton Canyon, indicated that the lake had crested near 20,000 years ago, and overflow had subsequently carved a canyon through unconsolidated alluvial deposits. This sequence of events led to the lowering of lake levels as water exited the basin to the east toward Soda and Silver Lakes.

A survey of the literature detailing the formation of desert pavements suggested that these geomorphic features were the products of wind erosion. Small-grained particles in desert areas had been removed by the wind, leaving behind larger particles (i.e., gravel), while accumulation of smaller particles downwind occurred in the form of sand dunes (such dunes are present across the basin from the East Rim site on the west slopes of the Cady Mountains) (see also appendix on Desert Pavement Formation in the final site report - Alsoszatai-Petheo 1975).

The significance of these observations to the association of desert pavement gravels and artifacts embedded in these gravels had been discussed by Hayden (1965). At the East Rim site (as indicated earlier) artifacts were found on the surface, embedded in the desert pavement, and in the stratum immediately below the desert pavement—yet no artifacts or flaked materials were found in the third, carbonate-rich stratum. These observations led me to conclude that the East Rim site presented evidence for a deflated stratigraphy in which only the first subsurface stratum had preserved the matrix originally associated with the artifacts encountered at the site.

Pollen samples were collected at the site from the carbonate-rich stratum, the first subsurface stratum, and surficial samples representing modern pollen rain. These samples were analysed by two laboratories whose reports were incorporated as an appendix in the final (1975) site report. Additionally, an effort was made to collect a representative sample of desert flora in the immediate vicinity of the site. This collection was submitted for classification to the Biology Department of California State University, Fullerton.

The modern pollen rain agreed with both the published data on the floral composition of a low elevation Moheve Desert modern life zone as well as the species collected near the site. The samples from the carbonate-rich basal stratum contained only oxidated (carbonized) microscopic organic remains labeled "fusain," while lacking any pollen. However, the first subsurface stratum contained abundant pollen which significantly differed in composition from the modern pollen rain.

Samples from this stratum produced high frequencies of pine pollen, mixed with pollen from other plants which are currently associated with water margin marshy areas, and with pollen of xeric forms (Drugg 1975). The final report on the analysis of pollen frequencies from these samples states that "these results exactly concur with those obtained by Mehringer (1967:174-78) at the Tule Springs site for the horizon dated 22,000 to 37,000 B.P." (Buge and Schoenwetter 1975:290). These authors further point out that changes in vegetation in the region preclude the possibility that these samples date any later than 14,000 B.P. (Buge and Schoenwetter 1975:290-291).

Palynological data, coupled with broader studies of the paleoecology of the Manix Basin during the Wisconsinan maximum (also reported as an appendix in the original report) indicate that stands of pine and fir in the higher elevations of the local mountains may have given way to parkland and cool open xeric vegetation near the East Rim site. Manix Lake was a relatively shallow lake covering over 200 square miles which may have been fringed by shoreline vegetation stretching to within a mile of the site. Paleontological remains suggest the presence of a rich aquatic ecosystem including various species of fish, fresh water shellfish, and a rich and varied avifauna. The terrestrial fauna conforms to the classic Rancholabrean dominated by camelids, although other non-desert adapted species are also common.

In addition to lithic procurement, discrete activity foci at the site seem to suggest a range of activities which included maintenance activities requiring the processing of softer materials through chopping, scraping, and cutting. Tentative interpretations of these activities might include cutting and whittling wood, cutting softer materials such as meat, processing plant foods, and preparing hides. Unfortunately, no organic materials were recovered which could directly corroborate these interpretations of lithic alteration and distribution patterns at the site. Natural transformational processes evident at the site—including weathering and decomposition at open sites coupled with severe wind deflation of the matrix—have clearly militated against the preservation of softer organic remains.

Feature One (as described above) suggests that this segment of the site may have served as a habitation area. In considering patterns of site utilization, it is also interesting to note that despite the relatively uniform availability of flakeable raw materials at the site, the majority of the lithic remains recovered at the site originated on the top and south-facing side of the site. This observation is surprising because the south side receives the maximum and most direct amount of sunlight in what today can be characterized as an intensely hot and harsh environment.

In assessing the significance of these data, together with the geomorphology and stratigraphy of the site and the changing availability of water and biotic resources at the site through time, it appears eminently reasonable to accept the conclusion that the East Rim site had been occupied contemporaneously with the presence of Manix Lake near the Wisconsinan maximum. This interpretation of the available data can be justified on two grounds. First, as the preceding discussions indicate, the data both contradict and fail to support significant components of the first three alternative hypotheses. Second (and by significant contrast), the last hypothesis provides most directly a single comprehensive explanation which can simultaneously and congruously account for all the observed data.

8.2.4 DISCUSSION AND ASSESSMENTS IN RETROSPECT

The East Rim site still presents two interrelated and unresolved issues. First, at the conclusion of the East Rim project it was clear that the materials recovered at the site bore similarities to the early phases of the Malpais/San Dieguito and Western Lithic Co-tradition sequences, yet it was not possible to demonstrate a systematic set of correspondences which would allow firm placement of the East Rim site into either sequence. Second, despite the interpretation of the genesis of the site's stratigraphy and chronology (discussed earlier), the presence of some surface artifacts left open the possibility that subsequent cultural transformations of the original record had altered, added to, or removed from the components of the original archaeological record—thus distorting the final reconstruction of prehistoric activities from the original period of site formation.

An inherent difficulty surrounding both issues is the lack of other comparably controlled and systematically excavated Manix Lake Lithic Industry sites. This problem can not be resolved by comparisons to impressionistic descriptions based on broad surveys (e.g., Simpson 1958, 1960), nor through more systematic yet limited sampling (Glennan n.d.) at one additional potentially similar site. The only solution appears to be a systematic, well controlled set of excavations of a number of potentially related sites. These data were not and are still not available. In the absence of such data, the very definition of a "Manix Lake Lithic Industry" remains a poorly-defined and untested construct. In view of this argument, we may still state that the East Rim site is distinctive when compared to other known cultural stages and sites in the region, yet we cannot test whether these differences are shared by other "Manix Lake Lithic Industry" sites or whether they are unique to this single site.

The second issue also embodies in it the uncertainties introduced by the possibility that the original record at the East Rim site may have been altered through subsequent cultural transformational processes. In light of local environmental shifts at the end of the Wisconsinan and during the Holocene, it seems reasonable to assume that subsequent cultural activities at the site, if any, were focused primarily or exclusively on lithic procurement activities. Thus subsequent cultural transformational processes which may have occurred at the site would have left a cultural signature characteristic of "quarry-workshop" activities rather than maintenance activities. This observation should not, however, be interpreted to mean that lithic procurement may not have formed a significant factor in the original activities represented at the site.

The use of the term "quarry/workshop" to describe a site can not and should not be viewed as synonymous with the conclusion that "quarry-workshops" represent a purely Holocene activity. In fact, we should expect that good sources of flakable material would always attract people. Thus, one of the remaining possibilities in the interpretation of the East Rim site and other similar sites is that the archaeological record includes lithic procurement, maintenance, or both types of activities from earlier periods upon which we find superimposed, purely lithic procurement activities by later groups visiting the site.

8.2.5 RECENT ADDITIONAL DATA

During the last ten years the development of cation-ratio (CR) studies of desert varnish by Dorn (1983) and Dorn et al., (1986) has opened new avenues by which the age of these sites can be established. Additionally, fieldwork headed by Bamforth (Bamforth et al. 1986; Bamforth and Dorn 1988; Bamforth 1990) has yielded additional data bearing upon the status of the Manix Lake Lithic Industry. The aim of this section is to review these developments in relation to the data and interpretations presented thus far from the East Rim site.

Given the lack of datable organic materials and the shallow stratigraphy at the East Rim site, the development of CR dating (capable of yielding age estimates directly from lithic materials) holds great promise in clarifying the chronological status of Manix Lake Lithic Industry materials. Since CR dating is still an experimental dating technique, a brief review of the nature and basic assumptions associated with CR dating is presented here as a source of background information for further discussion.

Cation-ratio dating is based on the presence, accumulation, and slow differential leaching of trace components of rock (or desert) varnish. Rock varnish is a thin coating of manganese and iron oxides and hydroxides, clay minerals, and trace elements that accrete on rock surfaces (Dorn 1983:49). Rock varnish frequently accumulates on exposed rock surfaces, pavements, and surficial lithic artifacts in desert areas.

> The premise of cation-ratio dating is that with time the leachable cations in varnish, such as Na, Mg, K, and Ca, are gradually replaced by and/or depleted relative to less mobile cations, such as Ti (Dorn 1983:50).

Dorn goes on to state that

> Cation ratios of varnishes from similar microenvironmental

> settings at a given site should indicate a relative-age sequence. Further, if the absolute ages of different surfaces can be determined (e.g., K/Ar dated volcanics), then an empirical relationship between age and cation ratios (called here a 'cation-leaching curve') can be established to provide estimates of the absolute ages of varnishes in the region where the relationship has been established (Dorn 1983:50-51).

Dorn notes the following limitations of the cation-ratio dating technique:

1) It should be stressed that age-determination techniques apply only to initiation of varnish formation, not necessarily to the age of the surface on which the rock varnish is deposited. The most important limitation is that the start of varnishing is not necessarily coincident with the exposure of the rock surface to subaerial processes. Too much alkalinity, too much acidity (e.g., from lichens), low concentrations of ambient clay minerals, high levels of organic matter, and/or competition from non-varnish-forming microorganisms may halt or impede the colonization of a rock surface by microorganisms that concentrate manganese (Dorn 1983:51).

2) Another important factor is the nature of the underlying substrate. The rate of varnish development on a smooth quartz or chert surface is slower than on more porous substrates. Weathering of quartz and chert does not produce clay minerals that are favorable for the initial colonization of varnish microorganisms... A smooth quartz or chert surface also does not favor the accumulation of clay minerals in microdepressions, a more suitable environment for microbial colonization (Ibid.).

3) Eolian abrasion can totally remove varnish from surfaces (Ibid.).

4) Lichens seem inimical to rock varnish in the western United States and Israel (Dorn 1983:53).

Initial calibrations and tests of the cation-ratio dating technique took place in the Negev Desert, Israel, and at several localities in the desert regions of southern California including Death Valley, Searles Lake, and in the Coso Range (Dorn 1983). In a subsequent publication, Dorn et al. (1986) presented the results of a systematic attempt to calibrate CR values using tandem accelerator mass spectrometry (TAMS). In this study a series of samples from 167 artifacts, obtained from six surface sites located in a line paralleling the Mohave River between Barstow and Baker, California, were tested by the CR dating method. Trace organic materials contained within the desert varnish of six samples from this region were also dated through TAMS radiocarbon analysis in an effort to calibrate the CR values to local microenvironmental conditions. Positive correlations were also noted between these calibrated values and K/Ar dates obtained on basaltic flows in the Coso Range to the north (Dorn et al. 1986:831).

The principal source of funding and the source of artifact samples used for calibration and testing in this study were derived from the Intermountain Power Project (IPP) headed by Douglas B. Bamforth (Dorn et al. 1986:833; Bamforth et al. 1986; Bamforth 1990). Bamforth is also one of twelve authors, and the principal archaeological investigator in the initial report on the TAMS calibrations of CR dates in the region (Dorn et al. 1986).

The nature of the artifacts, the criteria for their selection and their CR dates are introduced as follows:

> Most of the artifacts consist of chert flakes and cores collected in well-defined workshop areas. These types of artifacts were chosen because they could be reconstructed or refitted, thereby providing assurance of human origin and an internal check on the CR dates of individual artifacts (Dorn et al. 1986:831).

Artifacts were selected for testing by CR dating from six sites separated in a straight line by somewhat over 70 km. According to Bamforth and Dorn (1988:215) "one of these, CA-SBr-2100, is immediately adjacent to the Calico Mountains and is one of the localities where the Manix Lake Industry was first recognized."

Dorn et al. (1986) obtained a series of CR dates on single artifacts. Two of these six sites—CA-SBr-2100 and CA-SBr-3183—yielded a total of thirteen CR dates in excess of 10,000 years B.P. CA-SBr-2100 yielded dates of 10,200 ± 1,400, 11,500 ± 250, 12,600 ± 1,750, 14,100 ± 1,500, 15,500 ± 1,250, 20,900 ± 10,900, and 21,900 ± 4,600 years B.P.; while CA-SBr-3183 yielded dates of 11,500 ± 1,600, 11,500 ± 1,750, 12,600 ± 1,900, 13,800 ± 1,900, 13,800 ± 1,950, 15,800 ± 1,650, and 17,000 ± 4,600 years B.P. (Dorn et al. 1986:832).

Two sites, CA-SBr-2162 and CA-SBr-2223, presented special problems. "Aeolian abrasion of artifacts is evident at these sites, therefore, these determinations of varnish age [presented in Table 2 of their article] are useful only as minimum estimates of the age of artifact manufacture" (Ibid.). Finally, the last two sites, identified as CA-SBr-541 and "Silver Lake" (no site designation given), yielded only mid-to-late Holocene dates (Ibid.). These dates may be a little too young for Silver Lake artifacts, which have typically been dated to the early Holocene (cf. Bettinger and Taylor 1974; Moratto 1985:93-97), indicating perhaps either a delay in the onset of the formation of desert varnish, or the effects of unrecognized aeolian abrasion.

As a control for the older dates obtained on individual artifacts, Dorn et al. (1986:832) dated a series of refitted flakes and cores, stating that

> The CR dates are in five refitted flake-core sequences.

> Each sequence is given an age that represents an average (and standard error) of the mean CR dates of the artifacts in the sequence. The sequence ages are 21,400 ± 700 (n = 2), 14,200 ± 1,500 (n = 3), and 11,850 ± 900 years B.P. (n = 2) for site CA-SBr-2100 and 16,400 ± 840 (n =2) and 12,400 ± 1,100 years B.P. (n = 4) for site CA-SBr-3183. These standard deviations within these five sequences averages 7.1 percent—much smaller than the errors associated with the individual artifacts. This indicates that older CR dates are mutually consistent.

Additionally, these authors note that

1) Confidence [in the older CR dates] is provided by four varnish-radiocarbon [TAMS] calibration points in the late Pleistocene (Ibid.).

2) Varnish was sampled from the unworked surfaces of many of the artifacts. In all the artifacts this varnish had substantially older (more leached) CR's than the varnish on the adjacent worked surfaces (Ibid.).

3) CR dating only estimates the chronometric ages of subaerial rock varnish, thus inferring a minimum age of the underlying surface (Ibid.) [emphasis added].

4) ...special precautions must be taken to avoid contamination of the varnish sample. In a few cases of youthful CR dates, artifacts from the same refitted sequence have significantly different percentages of varnish surface cover, even though they have similar CR ages, because of microenvironmental disparities. This implies that unvarnished artifacts are not necessarily younger than varnished artifacts (Ibid.).

Despite their awareness that the CR dates represent only minimum dates (item 3, above), that there are a large number of factors which can effectively "reset the clock" (cf. Dorn 1983 and above), and that the onset of the accumulation of varnish is minimally dependent upon the onset of exposure to surface conditions, Dorn et al. (1986) expressed puzzlement at the range of dates obtained for the Lake Manix Lithic Industry specimens. In effect, they felt that an inherent contradiction was suggested by typological similarity between Manix Lake Lithic Industry specimens whose CR dates range from 2,100 to over 20,000 years B.P. Despite what they apparently considered inconsistent data, Dorn et al. (1986:832) go on to note that

> These CR dates suggest a tentative pre-Clovis human occupation of southeastern California, as only four CR dates are older than 12,000 years B.P. when two standard errors of the PIXE analyses are used to calculate the lower error margin of the CR dates. We realize that this is controversial, particularly because of the scarcity of well-dated and

> clearly human pre-Clovis material elsewhere in the New World and because of problems with claims for a late Pleistocene human occupation in the California Desert.

8.2.6 SITE CHRONOLOGY, TYPOLOGY AND GEOMORPHOLOGY

In the initial set of publications (Dorn 1983; Dorn et al. 1986) the application of CR dating to archaeological specimens had to be demonstrated. It seems, however, that the results obtained were viewed as "controversial" (Dorn et al. 1986:832) and in need of additional explanation. Therefore in subsequent publications the dates from the IPP sites were further supplemented by a small collection of Manix Lake Lithic Industry artifacts from elsewhere.

> A total of 20 surface artifacts identified as Manix Lake bifaces by F. Budinger were collected from alluvial fans in the eastern Calico Mountains by R. Dorn, D. Whitley, and F. Budinger and submitted for cation-ratio dating, along with one other biface identified by R. Simpson. These artifacts comprise a select, nonrandom sample of material intended to represent a range of macroscopic varnish development, from almost complete surface coverage to barely visible.
> The dates derived from these 21 artifacts range from less than 400 to 32,000 B.P., with 12 of them producing dates within the last 10,000 years and nine producing older dates. These results suggest that human occupation of the region may in fact have occurred in late Pleistocene (cf. Dorn et al. 1986), a point to which we return below, but they show fairly clearly that the material presently grouped together as the Manix Lake Industry does not represent a single period of human occupation in the region and therefore that the criteria used to group this material are not temporally meaningful (Bamforth and Dorn 1988:214) [reference in the original. Emphasis added].

This quote clearly states both the perceived nature of the problem, and their proposed solution. Having chosen a deliberately nonrandom set of specimens (typologically selected as specimens belonging to the Manix Lake Lithic Industry) showing a wide range of varnish development, they obtained a range of CR dates spanning from 400 to 32,000 B.P. Bamforth and Dorn (1988) chose to interpret these findings as an anomaly which they sought to explain as follows:

1) The range of CR dates obtained from "Manix Lake Lithic Industry" specimens represent the actual ages of specimens produced at different times (i.e., the CR dates are correct as reported, and represent actual ages of the specimens).

2) The CR dated specimens were produced by different peoples (presumably representing different cultures).

3) The spatial association of this mixed assemblage is fortuitous.

4) The resemblances between specimens of widely different ages is also fortuitous and due to the fact that they all represent the physi-

cal remains of unspecialized lithic procurement activities focused on localized lithic resources.

5) The extreme ages obtained on some of the specimens are not significant since the components to the "Manix Lake Lithic Industry" represent a mixed assemblage of different ages, and thus one can not argue for the existence of a "pre-Clovis" Manix Lake Lithic Industry.

In support of their interpretations Bamforth and Dorn (1988) draw upon the earlier work of Glennan (1976) to propose a set of hypothetical constructs. Bamforth and Dorn (1988:223) state that the Manix Lake Lithic Industry "...artifacts are quarry debris found _on_ desert pavements fortuitously above the 1,780 ft. level [the maximum level of Pleistocene Manix Lake], and that embeddedness in a pavement is no guarantee of great antiquity" [emphasis added]. Furthermore, they propose that "...isolates on the IPP sites were once part of clusters that have dispersed over time..." (Bamforth and Dorn 1988:220). Much of the data presented in their article in support of these hypotheses consists of statistical calculations based on selected samples of field observations obtained from the IPP sites. Unfortunately, after careful examination, many of their arguments remain unsupported and contradictory.

One contradiction stems from their explanation for the distribution of flaked materials, including embedded and isolated specimens.

> Experimental research by Bowers et al. (1983) indicates that surface clusters of flakes tend to disperse over time as the cumulative result of small, random movements of the individual artifacts they contain. Clusters on the IPP sites typically contain from 10 to 200 artifacts dispersed over areas of 5-15 m.2 The experimental results of Bowers et al. (1983) indicate clearly that clusters of this size and density could readily disperse to the point where they would be unrecognizable within a few thousand years, even given a very conservative rate of artifact movement (Bamforth and Dorn 1988:219) [citations in original].

This is an interesting and provocative hypothesis about the post-depositional behavior of lithic artifacts found in the Mohave Desert. Unfortunately, the application of the experimental data presented by Bowers et al., (1983) in these geomorphological contexts is less than convincing.

Bowers et al. (1983) conducted a series of well controlled and documented experiments demonstrating the mechanical dispersal of surface artifacts in Alaska. The article concludes that

> Even on the basis of this preliminary study, it is obvious that significant displacements can occur in only one season of freeze-thaw activity. Given the growing concern of archaeologists for precision in data recovery, brought about by increased awareness of the importance of spatial analysis, such displacements can prove to be major obstacles to accurate interpretation (Bowers et al. 1983:568).

The article further suggests that

> When dealing with sites suspected of having been affected by cold-climate processes, we must be prepared to ask ourselves the questions: (1) is our methodological precision commensurate with the accuracy inherent in a given archaeological context, (2) how much natural displacement may have occurred at a given site, and (3) in what ways will this distortion of the original spatial pattern affect our interpretations of the archaeological record? (Bowers et al. 1983:569) [emphasis added].

These seem to be highly appropriate caveats within geomorphic contexts dominated by permafrost, alternate freeze-thaw cycles, frost heave, and the action of needle ice as active geomorphic motive agents (Bowers et al., 1983)—but they appear to be singularly inappropriate as models to be employed in explaining artifact distributions at low elevation sites in the Mohave Desert.

Unfortunately, Bamforth and Dorn (1988) fail to mention or discuss these fundamental geomorphic differences. More importantly, lacking an explicit and appropriate mechanism which might account for artifact dispersal, their discussion of spatial distribution from the IPP data should be viewed as merely descriptive of what was found and not as predictive when stated in reference to an irrelevant set of geomorphic processes.

In their article, Bamforth and Dorn (1988) propose to evaluate the significance of the Manix Lake Lithic Industry based on the data gathered as part of a cultural resource management project conducted in the mid 1980s. Although Bamforth and Dorn cited Glennan's (1976) article based on a limited controlled surface collection at a Lake Manix Lithic Industry site, they failed to consider the vast, detailed data derived from controlled excavation by Alsoszatai-Petheo (1975).

Despite the fact that only a limited number of copies of this report were ever printed, the work was done under permit from the Bureau of Land Management's local office, which received a copy at the conclusion of the project. Other copies were also made available at that time to individuals, and to staff members of the San Bernardino County Museum. Two of the individuals mentioned by Bamforth and Dorn (1988) who provided samples of Manix Lake Lithic Industry artifacts were each in possession of a copy of the final report. Additionally, the final report is cited in a publication (Alsoszatai-Petheo 1981) about the Manix Lake Lithic Industry available for sale by the "Friends of Calico, Inc." at the Calico site. The final report is also cited by other authors whose work has focused on the issue of the early peopling of the Americas (e.g., Carter 1980; Payen 1982). It is difficult to understand how Bamforth and Dorn (1988) could have failed to find and incorporate the data from this report in their analysis.

In their article, Bamforth and Dorn (1988) adopted a hypothesis which assumes that the desert pavements associated with the lithic artifacts pre-dated the associated archaeological materials. Cores, flakes, clusters, and bifaces discussed in their article were interpreted as having been deposited on top of these pavements, while the embedding and the reported distributions were assumed to be a function of post-depositional movements modeled after Bowers et al. (1983). Appropriate geomorphological processes characteristic of the region were not presented.

By contrast, the analysis and interpretations presented in the final report on the East Rim site are intimately tied to the geomorphology of the region and to the geomorphic processes influencing the genesis of land forms and formation processes at the site. As has been noted in a previous section, a significant portion of the artifactual material at the East Rim site was found either embedded in the desert pavement, or entirely beneath the surface, below the pavement.

Field and laboratory data and analysis from the East Rim site confirm this last conclusion. Each square meter of the site was photographed as a color slide prior to excavation. Each square meter was also mapped in detail prior to excavation. All artifacts, including those recovered below the surface, were recorded in the field; and an analysis in the laboratory which included comparisons of field notes, laboratory notes, maps produced in the field, and oversized projections of the color slides were reconciled to each other. This procedure, more than any other, confirmed this last conclusion about the position of artifacts relative to the microstratigraphy of the site.

The interpretation of these data is intimately tied to an understanding of the geomorphological processes responsible for the formation of desert pavements. The final report from the East Rim site contains a lengthy appendix which reports and discusses the origin and formation of desert pavements.

Three principal mechanisms have been suggested which can account for desert pavement formation. Desert pavement formation can be attributed to the removal of fine sediments in the matrix by wind (deflation), or by rainfall and slopewash, and by the upward migration of gravel and larger particles due to alternate wet and dry cycles (see Cooke and Warren 1973:120-129). None of the sources in the published literature suggests any downward migration of larger (coarse) particles.

Of these three geomorphic mechanisms responsible for desert pavement formation, the principal and most persistent explanation offered by geologists and geomorphologists has been the mechanism of aeolian deflation (see Putnam 1964:304; Butzer 1971:193; Mabbutt 1977:123-126; Flint and Skinner 1977:208-209; Bloom 1978:316; Mears 1978:243; Sanders 1981:353-354; Strahler 1981:492; Leet et al. 1982:358; Plummer and McGeary 1982:258; Hay and McAlester 1984:357-358; Press and Siever 1986:215; Judson et al. 1987:358; Skinner and Porter 1987:320; Tarbuck and Lutgens 1987:304-306; Levin 1990:446-448). In his discussion of site formation processes, archaeologist Michael B. Schiffer (1987:217, 239) also acknowledges deflation as the principal mechanism responsible for the formation of desert pavements.

Bamforth and Dorn (1988:218, Table 2), using considerably less stringent controls, report that a significant proportion (over 20 percent) of the artifacts from the IPP sites were found more than fifty percent buried. This burial they attribute to redeposition of the artifacts due to slopewash.

As noted earlier, slopewash was clearly not a factor at the East Rim site. Flake concentrations and their parent cores were found together within small areas, the desert pavement was intact and unbroken, the degree of slope over most of the site was minimal (2 to 4 degrees), and despite these conditions a significant proportion of the recovered artifacts were not only embedded, but actually buried beneath the desert pavement at this site.

The matrix containing the buried artifacts was the source of the pollen samples which were correlated to carbon-dated core samples dating between 22,000 and 37,000 years B.P. As in the IPP sites and the 21 Manix Lake Lithic Industry artifacts reported by Bamforth and Dorn (1988), surface and buried artifacts from the East Rim site failed to show any appreciable difference in typology or manufacturing technique. However, unlike the data from the IPP sites reported by Bamforth and Dorn (1988), the East Rim site produced a variety of artifact types (not just "crude bifaces") in recurrent spatial association, in addition to lithic workshop remains such as cores, flakes, debitage, and clusters.

On the basis of the pollen associations and the location of the Manix Lake Lithic Industry sites above the 1,780' shoreline of Pleistocene Manix Lake, the archaeological remains at the East Rim site had been tentatively assigned a date of circa 20,000 B.P. (Alsoszatai-Petheo 1975:149-153). The association of artifacts with the pollen samples was justified on the basis of the buried position of the artifacts, coupled with the lack of mechanisms which could account for the downward migration of larger particles, and the normal interpretation of the geomorphic processes responsible for desert pavement formation (i.e., deflation, slopewash, and upward migration of larger particles).

In comparing these data with the data from the IPP sites we must realize that unlike the situation at the East Rim site, the artifacts recovered from the IPP sites originated from a range of surface conditions including varying degrees of slope, and varying degrees of pavement conditions. This mixed pattern did not yield any obvious relationships to the majority of the CR dates, although there does appear to be a weak inverse relationship between degree of burial and age for artifacts found in association with well-knit ("armored") desert pavements (Bamforth and Dorn 1988:218). When we join this observation with the geomorphic mechanisms responsible for desert pavement formation and the realization that exposure to the air is necessary before desert varnish formation begins, the apparent inconsistencies yielded by the IPP data can be easily understood.

To recapitulate, Manix Lake Lithic Industry artifacts do indeed date to the late Pleistocene. In their original setting these artifacts were produced from scattered nodules of suitable raw material found on the surface of the Yermo alluvial fan. Although the climate at this time was dry, xeric conditions were not as extreme as they are today. Many of the dry washes dissecting the alluvial fan today were undoubtedly formed or enlarged in Holocene times. The local topography was probably dominated by more vegetation and less variation in relief. Artifacts at these sites became incorporated in a shallow stratigraphy as a result of a combination of factors including human activity, aeolian deposition, and slopewash. Some of the artifacts probably were never buried.

Towards the end of the Pleistocene and especially during the Holocene, major floral changes took place, and renewed erosion—especially deflation—began to re-expose the artifacts. Exposure to the air was not immediate for all specimens. Furthermore, some specimens became re-buried when water erosion formed dry washes scoured by occasional torrential summer downpours, altering the slope on which artifacts rested. At such times when individual artifacts were exposed on the surface, the accumulation of desert varnish began. Throughout this period some specimens remained buried, while others in increasing numbers were exposed on the surface as part of newly emerging desert pavements.

The range of observed desert varnish development on the 21 specimens of Manix Lake Lithic Industry artifacts reported by Bamforth and Dorn (1988:214) parallels the observed range of variation of the dates obtained from these specimens (from 400 to 32,000 years B.P.). Far from contradicting the data and interpretations from the East Rim site, these data confirm the pollen dates and the dates obtained from shoreline tufa samples; and they lend direct support for the original site formation hypothesis and the chronological interpretation of the East Rim site. Given this hypothesis, all the perceived inconsistencies produced by the IPP project fall away, and the incongruities of typology and chronometric data are explained everywhere with a single, parsimonious explanation.

If there is an "irony" associated with this situation, the irony lies not with the incongruities of typology versus chronometric dating, but with the apparent reluctance of researchers to accept the validity of their own observations when they fail to agree with their *a priori* expectations. As a postscript to this analysis, a recent article by Dorn et al. (1992) presents an analysis of AMS dates obtained from organic inclusions in varnish from Lake Manix Lithic Industry artifacts yielding dates up to 26,000 years B.P. After carefully addressing alternative explanations for these dates, Dorn et al. (1992:145) conclude that "the simplest hypothesis is to view these pre-Clovis radiocarbon dates as minimum ages for the exposure of flake scars by humans."

Beyond the data presented, and the history of restudy and confirmation of the conclusions presented in the original study of the East Rim site, I also believe that this example can serve to illustrate the dynamics and nature of the problems which researchers have, and will continue to encounter so long as research into the Pleistocene archaeology of the Americas continues to proceed without the benefit of a comprehensive, explicit, and broad-based theoretical framework which opens reasonable avenues for the interpretation of new, incoming field data. The synthesis of the data, the model, and the model output presented in this work represent a first step toward the generation of such a theoretical framework, which like the evaluation of field data, will ultimately depend upon constructive dialogue between open-minded members of the professional community.

8.3 ARCHAEOLOGICAL IMPLICATIONS OF THE MONTE VERDE AND THE EAST RIM SITES

In this chapter I have tried to convey a set of fundamental observations and alternative perspectives by means of field-specific data recovered through a specific research strategy. In the cases presented, I felt that this research strategy was clearly evident. The Monte Verde site—though still under analysis—has been sufficiently documented to support such a perspective. In the case of the East Rim site, I had a sufficient familiarity with the field data to be able to respond authoritatively in a professional dialogue about the significance of the Manix Lake Lithic Industry.

The relationship of the evidence presented by the Monte Verde site to the model's predictions for the Crisis and Specialization periods has already been discussed. The East Rim site, on the other hand, represents a Saturation period site. According to the predictions of the model output for this period, such sites (often located along ecotonal areas, such as major waterways) should provide evidence of more intensive or repeated use of local resources. Despite the lack of ecofacts and artifacts made from perishable materials at the East Rim site, I believe that the data presented by the site conform to the archaeological criteria specified in section 6.3; and to the predictions of increased use of durable and more readily recognizable artifactual types, resulting in a greater level of "visibility" in the archaeological record (section 6.2).

Although the predicted pattern characterizing the material culture of this period emphasizes reliance on artifacts made from perishable materials, there are no macrofossil remains left at the East Rim site; and thus we are left to extrapolate their presence from the stone tools at the site, which strongly suggest a variety of maintenance activities designed to shape wood or bone. In this sense, it is indeed unfortunate that the wealth of lithic remains, coupled with evidence for lithic procurement activities, at Manix Lake Lithic Industry sites should have indirectly influenced the opinion of researchers; and fueled the arguments which led to their common dismissal as recent and irrelevant "quarry-workshop" sites.

Looking beyond the East Rim and Manix Lake Lithic Industry sites, we may suggest that these sites merely represent a local manifestation of a much broader pattern of Pleistocene lake shore adaptation which should have left a recognizable archaeological record elsewhere in the Great Basin. Perhaps at other locations archaeological sites will be found in which perishable materials, artifacts, ecofacts, and even human skeletal remains will be found. Tentatively, one such find from the south end of the Manix Basin might be a fragment of human temporal bone which was discovered in the late 1970s by paleontologist George Jefferson as part of curation of a Rancholabrean collection obtained from Schuiling Cave (Jefferson 1983).

By contrast to the East Rim site, the Monte Verde II site belongs to a much later period of Proto-American prehistory. The Monte Verde site is located in South America, in a wet habitat near the Pacific Ocean. The site is rich in well preserved perishable artifacts, architectural features, and ecofacts. These riches of organic remains have permitted a degree of reconstruction of the details of life at a settled, terminal Pleistocene-age village, which are unparalleled anywhere else in the Americas. Indeed, the very existence of a settled way of life at such an early period in American prehistory had not been suspected until the discovery of the Monte Verde site.

At first glance, there does not seem to be any convincing reason to consider these two sites together. Nevertheless, these two sites can serve to illustrate some interesting commonalities in relation to Proto-American archaeology.

1) Both sites are found in association with plentiful naturally occurring deposits of raw materials (wood and plant materials at Monte Verde, and flakeable stone at the East Rim site). These similarities have required the application of specialized tests at each site to establish the artifactual status of the recovered cultural remains. Similarly, both sites similar to the East Rim site (Bamforth and Dorn 1988), and the Monte Verde site (Lynch 1990), have been rejected on the basis of similar types of faulty arguments (see Dillehay and Collins 1991, and section 8.2.6, above, for rebuttals).

2) Since there are no known early American sites containing evidence which might parallel the record of the Monte Verde site, it is not clear how other such sites might have been or will be treated by other archaeologists. On the other hand, in the case of the East Rim site, the approach employed by Glennan (1974, 1976), and Bamforth et al. (1986) at similar sites, suggests that there might be a real tendency by some archaeologists to underestimate the significance of such sites resulting in the application of inappropriate field and analytical techniques. The use of such techniques tends to draw attention away from the critical data which needs to be collected, and these gaps ultimately lead to the proposal of spurious conclusions.

3) Comparing the lithic and organic remains yielded by the Monte Verde and the East Rim sites, it becomes obvious that each site presents different set of strengths. It is very likely that neither site presents the full range of technologies which its occupants employed in prehistoric times. At the same time, each site has yielded a rich archaeological record which deserves our full and careful attention, even if neither site conforms to a predetermined set of standards which include biases derived from the informational content of previously excavated sites.

In fact, one of the forceful lessons provided by the Monte Verde II site is the importance of keeping in mind the distorting effects of natural transformational processes which have severely altered the record of most other early sites. It is no more appropriate to dismiss the validity of the evidence provided by the East Rim site due to a lack of perishable remains, than it would be appropriate to dismiss both the Monte Verde and the East Rim site's chronological age and cultural significance to Proto-American prehistory because neither site was found stratigraphically (and therefore geographically) located below a bona fide Clovis site.

4) But perhaps the most compelling reason to include a case study review of these two sites concerns the need to apply reliable and thorough analytical methods, including the method of multiple working hypotheses, in the investigation of all sites, and particularly all

potentially early sites in the Americas. When such methods are integrated with an explicitly formulated theoretical framework, alternative possibilities in the interpretation of field data become possible, and our assumptions and expectations, rather than the validity of empirical data can become the focus of subsequent discussion and testing.

Chapter Nine

EVALUATION AND FUTURE DIRECTIONS

In the introduction to this work I observed that the study of the initial peopling and early prehistory of the Americas has been fraught with difficult and controversial issues. The use and application of isolated gains realized through painstaking research at archaeological sites has been severely limited by an apparent inability of members of the field to successfully generalize their findings into a broad and comprehensive synthesis—despite numerous previous attempts. In this work I pursued a different course by explicitly formulating a theoretical framework derived from a broad based, independent, holistic perspective.

It was pointed out in chapter two that the adoption of a holistic model capable of integrating data and principles derived from outside the traditional archaeological domain of Proto-American prehistory required the active use of a biocultural approach to modeling. This approach presented some special concerns.

1) Some of the data and principles used in generating the model output would be likely to pose special problems since they originated from "space-like" frame sciences, while their intended application was aimed at the explanation of cultural dynamics in a "time-like" frame (cf. Dunnel 1982).

2) The principal method which was to be employed to overcome the incompatibilities separating "space-like" frame from "time-like" frame approaches was the use of the principle of uniformitarianism, coupled with the use of general comparative ethnographic analogy (cf. Gould 1978; Binford 1967, 1978; Howell 1976).

Additionally, the model specifically proposed that the principles and data of two or more independent disciplines or subdisciplines should be focused simultaneously on the solution of individual or interrelated sets of problems. This approach using multiple independent lines of reasoning was considered to be an effective means of solving difficult broad based problems which any one field had thus far failed to resolve. Examples of such an approach to problem resolution in anthropology could be found in the works of Sapir (1916), and in the theoretical foundations and methods of modern paleoanthropology (cf. Wolpoff 1980, and Strauss 1991).

It was further proposed that the use of this model in generating a model output of Proto-American prehistory also required the adoption of four specific guidelines which held that:

1) The events which characterized Proto-American prehistory should be viewed as a single set of realities explainable from a biocultural perspective which integrates cultural data with relevant data from physical anthropology, and the natural sciences;

2) The model output of Proto-American prehistory should be generated independently from the archaeological data recovered from early New World sites so as to avoid committing tautological errors in reasoning;

3) The model output of Proto-American prehistory should address a broad range of issues from a diachronic perspective. These issues

should include considerations of the source, movement, population numbers, and cultural dynamics of the initial immigrants, and their descendants. Given the limitations imposed by the preceding guideline, the relevant model outputs should be broad based, and comprehensive; and they should be derived from a consideration of various sources including elements of general comparative analogy drawn from small-scale societies, human biology, demography, paleoanthropology, Old World prehistory, and paleoenvironmental studies; and

4) The holistic model output of Proto-American prehistory should be capable of generating relevant hypotheses amenable to subsequent testing through the totality of the empirical data obtained from Proto-American archaeological sites in the Americas.

As a result of the adoption of this model, chapters three and four of this work purposefully diverged from the normal, highly specific, and archaeologically based data and arguments which have typically characterized past syntheses of early American prehistory. Instead, these chapters presented a variety of discussions and a review of theoretical constructs, data, and conclusions regarding the biocultural, demographic, and ecological dynamics of small-scale societies; chronological age range estimates for initial entry derived from a variety of genetic, demographic, and paleoanthropological projections; the general geographic, paleoclimatological, and paleoenvironmental setting of the Mid-Wisconsinan; and a review of Mid-Wisconsinan culture-historical reconstructions drawn from the prehistoric record of the Far Eastern Pacific rim. These discussions and their associated conclusions were used to generate a generalized model output (chapter five), followed by a preliminary set of projections and specific hypotheses concerning the early prehistoric record of the Americas (chapter six). The last two chapters provided a review of early sites from the Americas which were reported to date to the time range which formed the focus of the model output.

9.1 ISSUES IN MODELING AND PROBLEM DEFINITION

From the perspectives of this work it seems that the definition of the nature and scope of the problem of Proto-American prehistory must come first, before we can evaluate data, choose our methods, establish criteria for valid evidence, or evaluate our results. One fundamental problem faced by all archaeology stems from the disparities which separate "time-like" and "space-like" frames. Whereas our ultimate focus concerns the reconstruction of past dynamic phenomena including cultures and cultural processes, our data base is limited to what can be viewed as essentially a static proxy record of the past.

Another difficulty stems from the manner in which we choose to limit the focus and scope of legitimate phenomena for systematic study within archaeology. Thus, the incorporation of information from other disciplines and subdisciplines into archaeological research often comes in the form of techniques such as palynology, remote sensing, or chronometric studies, while broader applications of the full potentials presented by the data and theory of related disciplines to the problems of archaeology remain relatively unfamiliar and foreign to most archaeological constructs in Proto-American prehistory.

Endowed with an empirical data base rich in culture-specific details, the generalized principles and constructs of other fields may seem irrelevant or distant from the immediate goals pursued by typical archaeological research projects. Archaeological data typically yields specific answers to limited but concrete questions. On the other hand, other fields can supply alternative explanations, and suggest additional answers to broader contextual and processual issues, which can be equally helpful to the solution of archaeological research problems.

Unfortunately, the advantages derived from a broader reliance on the insights obtained from related fields often requires active participation by the archaeologist in the formulation of problems, and in the use and application of the methods and theories employed by other fields. In adopting a broader approach to problem definition, the archaeologist can no longer limit himself or herself to the role of the informed consumer of the technical aids provided by other fields, but must instead become an active participant.

Ideally, the best solution to the difficulties posed by the incorporation of the strengths of related fields within the framework of active archaeological research is to approach specific problems through the collaboration of a multidisciplinary team. This approach is now commonplace in paleoanthropological field research. On the other hand, when the focus of research is aimed at resolving problems which have traditionally formed the domain of prehistoric archaeology, then the archaeologist will have to rephrase traditional archaeological problems in such a way that the methods and theories of other fields can be effectively applied in obtaining valid results. This last requirement embodies the challenge posed by the model of multiple independent lines of research employed in this work.

9.2 ANALYSIS AND RECOMMENDATIONS FOR FUTURE RESEARCH

As I have stated earlier, the debate surrounding the initial peopling and early prehistory of the Americas has been characterized by the use of archaeological data through the process of inductive generalizations [cf. Krieger 1964; Haynes 1964, 1969; MacNeish 1976; and Mosimann and Martin 1975]. Such an approach begins with data collection, and is followed by the generation of a synthesis which forms the basis of interpretations of broad scope. By contrast, in this work I have tried to define some relevant issues, and using a model of multiple independent lines of research, I have tried to address these issues separately from the early archaeological record of the Americas.

The principal outcome of the application of the model of multiple independent lines of research has been the generation of a model output which is derived explicitly and separately from the data base of Proto-American prehistory which it is intended to predict. This approach provides new perspectives which will hopefully point to new solutions, and new avenues of future research.

Beyond the heuristic dimensions of the model, the model output presented in this work clearly indicates that:

1) American prehistory extends well into the Mid-Wisconsinan, long before the assumed arrival of the first Clovis hunters.

2) The processes of migration, population growth, and dispersal proceeded much more slowly than had been modeled by Mosimann and Martin (1975).

3) Slower dispersal and population growth rates produced a thin and patchy pattern of occupation by generalized foragers who preferentially occupied favorable habitats and ecotonal zones. Furthermore, these patterns probably continued for approximately twenty thousand years prior to the Wisconsinan maximum.

4) Foraging techniques and strategies throughout much of the pre-Covis occupation of the Americas focused on prey types which could be easily accessed with a minimum use of tools or social cooperation in prey capture.

5) The shift towards active and regular hunting of larger game took place only towards the end of the Wisconsinan; and this shift represented a set of cultural responses to resource depletion caused by climatic and ecological changes associated with the end of the Pleistocene.

Although chapter seven of this work attempted to provide a survey of the reported early sites which correspond to the broad temporal subdivisions of the model output, this survey does not provide a true test, but simply a demonstration of the applicability of the model output to the existing published archaeological record spanning this period. The model output and the hypotheses presented in chapter six should form the basis by which more specific archaeological hypotheses can be tested at future excavations.

The archaeological record spanning these early millennia of American prehistory was recovered without reference to a broad based model output [as Dincauze (1984:282) so aptly put it, "there exists no 'school' of Pre-Clovis advocates"]. Consequently the collection, analysis, and reporting of data at these sites tends to present an idiosyncratic and uneven record. Under these circumstances it is difficult to accurately assess the validity of the predictions of the model output presented in this work. The two case studies presented in chapter eight were presented partly in order to outline two early sites which provide examples of more reliable research strategies and methods. However, the most reliable test of the validity of the model output will begin when researchers excavating early sites begin to actively address and test the predictions of the model output presented in this work. Hopefully this model output will provide a source of potential multiple hypotheses which will be tested through direct archaeological work.

In his discussion of the method of multiple working hypotheses, Platt (1964:352) states that

> ...you can catch phenomena in a logical box or in a mathematical box. The logical box is coarse but strong. The mathematical box is fine-grained but flimsy. The mathematical box is a beautiful way of wrapping up a problem, but it

> will not hold the phenomena unless they have been caught in a logical box to begin with.

Using Platt's terminology, this work has been aimed at "catching the phenomena" of Proto-American prehistory in a "logical box" constructed from mutually interactive independent lines of research.

The archaeological data which will be needed to test and support or modify the broad components and details of the model output presented in this work will only emerge piece by piece through careful and painstaking analysis of individual sites discussed in reference to the model output. This goal can not be accomplished by a single author or in a single publication. It represents the concerted, published dialogue of the whole community of archaeologists, each of whom will only address portions of the overall predictions of the model based on their own field and laboratory data.

Undoubtedly the model and the model output will undergo many changes in the future—but this is only as it should be. The introduction of a new paradigm is only the beginning of a new phase of work and discovery. Yet at the same time, new paradigms are designed to introduce new perspectives and new avenues of research. I believe that the next few decades in the study of the initial peopling of the Americas will be fruitful and exciting as well as frustrating at times. The present model output may survive the test of time, or it may ultimately be superseded by better theoretical constructs. In either instance, however, it is my sincerest hope that in formulating this work I will have contributed to the dialogue among fellow archaeologists and that this work will prove useful and thought provoking to all amongst us who are still searching and striving to unravel the story of the first Americans.

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Appendix 1

SUMMARY OF GENETIC DATA

Appendix 1 - Genetics Literature Survey - Table 1a

GROUP(S) SURVEYED	ABO	MNSs	Rh	Fy^a	Le^a	Di^a	P	K	Lu^a	Jk^a	Js^a	Ba^a	Wr^a	Other	REFERENCE
Mapuche Indians from Cautin Province, Chile	X	X	X												Sandoval and Henckel 1954:324-329
Araucanian (Pehuenche and Mapuche) Indians, Temuco, Chile														Hpl	Nagel and Etcheverry 1963:187-188
Alacaluf, Mapuche and Atacameño Indians of Chile	X	X	X	X	X	X	X	X	X	X			X	Hpl, Tf	Matson et al. 1967:157-194
Eleven tribes in the Mato Grosso region of Brazil	X	X	X	X			X	X	X						Pantin and Junqueira 1952:395-405
Caingang (Rio Grande do Sul, Brazil) and comparative South American	X	X	X			X									Salzano 1961:391-404
Pemon (Caribs) Southwestern Venezuela	X	X	X	X	X	X	X	X		X				Hpl	Layrisse et al. 1962:411-420
(10 pops) Paez, Ica, and Tunebo, Colombia; Warrau, Venezuela	X	X	X	X	X	X	X	X		X					Layrisse et al. 1963:36-55
Continental Caribs, Venezuela	X	X	X	X	X	X	X	X		X					Layrisse et al. 1963:140-166
Aweikoma, Guarani, and Caingang, Santa Catarina, Brazil	X	X	X	X		X	X	X					X		Salzano 1964:91-106
Aymara, Chama, Tacana Chacobo, Itonama, Moro, Siriono, Bolivia	X	X	X	X	X	X	X	X	X	X			X	Mia, Vw, Hpl, Tf	Matson et al. 1966:13-34
Xavante, Mato Grosso region, Brazil	X	X	X	X	X	X	X	X		X			X	Vw	Gershowitz et al. 1967:502-537

Appendix 1 - Genetics Literature Survey - Table 1b

GROUP(S) SURVEYED	ABO	MNSs	Rh	Fy^a	Le^a	Di^a	P	K	Lu^a	Jk^a	Js^a	Be^a	Wr^a	Other	REFERENCE
Chamacoco, Guayaki, Moro, Toba, Chulupi, Lingua, Yam Lengua, in Paraguay	X	X	X	X	X	X	X	X	X	X				Hpl, Tf	Matson et al. 1968:81-98
Eight northern Brazilian Indian populations	X	X	X	X	X	X	X	X	X	X				Hpl, Tf	Matson et al. 1968:303-330
Eight Argentinian Indian populations	X	X	X	X	X	X	X	X		X				Hpl, Tf	Matson et al. 1969:61-84
Nine Paraguayan Indian populations	X	X	X	X	X	X	X	X						Gm, Hpl, G6PD	Brown et al. 1974:317-344
Eight South American tribes (various locations)	X	X	X	X		X	X			X				Dermatoglyphics	Rothhammer et al. 1977:51-60
Cayapo and Kraho (Ga) Central Brazilian Plateau	X	X	X	X	X	X	X			X				Gc, AcP, PGM	Salzano et al. 1977:337-348
Eleven South American and one Central American population		X	X	X		X				X				Gm, Km	Gershowitz and Neel 1978:289-302
Pano, Baniwa, Kanamari, of the central Amazon Basin, Brazil														Hp, PGM, Polypeptides	Mohrenweiser et al.1979:237-246
Parakana, Central Brazilian plateau	X	X	X	X		X		X		X				G6PD, Hp, 6-PGD,Tf, Cp, Gc	Black et al. 1980:119-132
Ten South American Indian tribes (various locations)														ESD	Mestriner et al. 1980:95-101
Mixed Indian/non-Indian populations, Porto Alegre, Brazil	X	X	X											G6PD, Gc, Tf, Hpl	Franco et al. 1982:127-132

Appendix 1 - Genetics Literature Survey - Table 1c

GROUP(S) SURVEYED	ABO	MNSs	Rh	Fy^a	Le^a	Di^a	P	K	Lu^a	Jk^a	Js^a	Be^a	Wr^a	Other	REFERENCE
Nine South American Andean or near-Andean tribes														Dermato-glyphics	Murillo et al. 1977:45-50
Mixed populations from northern Peru	X	X	X	X	X	X	X	X	X	X		X		Sickle cell anemia	Ellis et al. 1963:26-52
Quechua, Colorado, Jivaro, Secoya, and Cayapa, Ecuador	X	X	X	X	X	X	X	X	X	X		X	X	Hpl, Tf	Matson et al. 1966:51-70
Ten Peruvian Amer-indian groups	X	X	X	X	X	X	X	X	X	X			X	Hpl, Tf	Matson et al. 1966:325-350
Shuar (Jivaro) of Ecuador and Peru														PGM,6-PGD Hpl,Gc,Tf 9 others	Goedde et al. 1977:419-426
Atacameño of north-ern Chile	X	X	X	X		X		X							Rothhammer et al. 1984:243-247
Icana of north western Brazil	X	X	X	X	X	X	X	X		X				Gm,Km,Tf, Al,PGM & others	Salzano et al. 1986:3-14
Five archeological sites 5,458 B.C. to 1,300 A.D.; Chile-Peru border region														cranial (non-metric) variab.	Rothhammer et al. 1984:157-162
Peruvian Indians (unspecified)														PTC testing	Paolucci et al. 1971:427-430
Diegueño, Southern California, U.S.A.	X	X	X	X			X		X						Pantin and Kallsen 1953:91-96
Nine Mexican and three Guatemalan Amerindian popula-tions	X	X	X	X	X	X	X	X	X	X				He, V-antig. Mia	Matson and Swanson 1959: 49-74

Appendix 1 - Genetics Literature Survey - Table 1d

GROUP(S) SURVEYED	ABO	MNSs	Rh	Fy^a	Le^a	Di^a	P	K	Lu^a	Jk^a	Js^a	Be^a	Wr^a	Other	REFERENCE
Four Lacandon Maya groups, Yucatan, Mexico	X	X	X	X	X	X	X	X	X	X				Mia, Vw	Matson and Swanson 1963:319-334
Twenty-nine Maya communities in Guatemala	X	X	X	X		X	X	X		X					Tejada et al. 1961:319-334
Aymara and Quechua, Puno, Peru	X	X	X	X	X	X	X	X		X	X				Best et al. 1962: 321-329
Tzotzil and Zincantan Maya, Chiapas, Mexico	X	X	X	X	X	X	X	X	X	X	X	X	X		Matson and Swanson 1963:1-14
Maya (unspecified), Guatemala	X	X	X	X	X	X	X	X	X	X	X	X	X	Mia, Vw	Matson and Swanson 1963:301-318
Jicaque, Paya, and Lenca of Honduras	X	X	X	X	X	X	X	X	X	X	X	X	X	Mia, Vw	Matson and Swanson 1963:319-334
Chorotega, Subtiaba, Miskito, Sumo, and Rama, Nicaragua	X	X	X	X	X	X		X	X	X		X	X	Mia, Vw	Matson and Swanson 1963:545-559
Maya (unspecified), Belize	X	X	X	X	X	X	X	X	X	X	X	X	X	Mia, Vw	Matson and Swanson 1964:271-284
Boruca, Bribri, Cabecar, and Terraba tribes, Costa Rica	X	X	X	X	X	X		X	X	X	X	X	X	Mia, Vw, V-antig.	Matson and Swanson 1965:107-122
Nine Indian groups from Belize, Costa Rica and Panama	X		X											Hp, Tf, and others	Matson et al. 1965:123-130
Guaymi, San Blas Cuna, and Choco of Panama	X	X	X	X	X	X	X	X	X	X		X	X		Matson and Swanson 1965:413-426

Appendix 1 - Genetics Literature Survey - Table 1a

GROUP(S) SURVEYED	ABO	MNSs	Rh	Fy^a	Le^a	Di^a	P	K	Lu^a	Jk^a	Js^a	Be^a	Wr^a	Other	REFERENCE
Twelve Mexican Amerindian populations	X	X	X	X		X	X	X							Cordova et al. 1967:55-66
Eleven south and central Mexican populations														Al^{me}	Lisker et al. 1971:119-124
Two Guatemalan Highland populations														Al^{me}	Johnston et al. 1973:27-30
Two Tlaxcalan Amerindian populations, Mexico	X	X	X	X	X	X	X	X		X				Ge, Tf, Hp, AcP, PGM	Crawford et al. 1974:251-268
Eight Colombian Amerindian populations	X	X	X	X	X	X	X	X						AcP, PGM, 6-PGD,AK, & others	Kirk et al. 1974: 301-316
Thirteen Amerindian tribes, Mexico	X	X	X	X		X	X	X						Hp, Tf, G6PD, & others	Roychoudhury 1975:449-454
Twelve North American populations (various locations)														Al^{na},Al^{me}	Schell et al. 1978:111-118
Tlaxcaltecan Indians, Mexico	X	X	X	X	X	X		X	X				X	Yta,Vel, Ge, Gya, & others	Crawford et al. 1979:401-412
Waorani, Ecuador														Gm, Km, AcP, GLO, & others	Larrick et al. 1985:445-453
Pima, Papago, Zuni, Hopi, Walapai, Apache, and Navaho southwestern U.S.A.														Gm, Km	Williams et al. 1985:1-19
Japanese, Ainu, Pima (U.S.A.), Australian Aborigines, and various other groups														Dental (crown) traits	Hanihara 1979: 125-134

Appendix 1 - Genetics Literature Survey - Table 1f

GROUP(S) SURVEYED	ABO	MNSs	Rh	Fy^a	Le^a	Di^a	P	K	Lu^a	Jk^a	Js^a	Be^a	Wr^a	Other	REFERENCE
Miskito, Sumo, and Rama of Nicaragua														PTC tasting	DeStefano and Mollari 1976:371-374
Navaho, Arizona, U.S.A.														Hpl	Parker and Bearn 1961:106-108
Apache and Navaho, Arizona, U.S.A.														Dermato-glyphics	Flikinger and Yarbrough 1976: 117-122
Zuni, Pima, and Papago, Arizona, U.S.A.	X	X	X	X	X	X	X	X		X				Hpl,Gcl, PGM, me Tf, Al	Workman et al. 1974:119-132
Ute, Utah; and Blackfeet, Montana, U.S.A.	X	X	X												Matson and Piper 1947:357-368
Chippewa, Minnesota, U.S.A.	X	X	X	X			X	X							Matson et al. 1954:413-426
Cherokee, North Carolina, U.S.A.	X	X	X	X		X	X	X	X					He, Hu	Pollitzer et al. 1962:33-43
Chocktaw, Missis-sippy, U.S.A.														Cerumen types	Martin and Jackson 1969:677-678
Yakima, Okanogan, and Swinomish, Washington, U.S.A.	X	X	X	X											Hulse 1957:235-246
Blackfeet, Alberta, Canada	X	X	X	X	X		X	X	X	X					Chown and Lewis 1953:369-383
Ojibwa (Algonquian), Canada														Al, Hpl, Gm, PGM, & others	Szathmary et al. 1974:49-66

Appendix 1 - Genetics Literature Survey - Table 1g

GROUP(S) SURVEYED	ABO	MNSs	Rh	Fy^a	Le^a	Di^a	P	K	Lu^a	Jk^a	Js^a	Be^a	Wr^a	Other	REFERENCE
Dogrib, N.W. Territories, Canada														6 serum proteins, 8 enzymes	Szathmary et al. 1983:249-254
Eskimos of western Alaska	X	X	X												Matson and Roberts 1949:109-122
Alaskan and Siberian Eskimos and other Siberian populations	X	X	X	X		X	X	X		X			X	Coe, Gye, Yte, He, Cp, ESD	Crawford et al. 1981:167-185
St. Lawrence Island Eskimos, Alaska	X	X	X	X		X	X			X				AcP, AK, ESD, Gm, & others	Ferrell et al. 1981:351-358
Two Alaskan Eskimo populations														Cerumen types	Pawson and Milan 1974:431-432
Eskimos and Aleuts, Alaska														Cerumen types	Bass and Jackson 1977:209-210
Eskimo and Aleut populations of U.S., Canada and Greenland														Craniometric meas.	Zegura 1975:271-284
Nganasans (Tanghi) Taimir Peninsula, Russia	X	X	X	X		X	X	X						Hpl, PGD, PGM	Karaphet et al. 1981:139-145
Mongolians, Koreans, and Zhuang Chinese	X	X	X	X		X		X		X					Goedde et al. 1984:277-284
Siberian peoples (various), Russia														Cranio/Anthr/measurements	Alexseev 1979: 57-90
Prehistoric vs. modern populations in China														Tooth size	Brace et al. 1984:485-516

Appendix 1 - Genetics Literature Survey - Table 1h

GROUP(S) SURVEYED	ABO	MNSs	Rh	Fy^a	Le^a	Di^a	P	K	Lu^a	Jk^a	Js^a	Be^a	Wr^a	Other	REFERENCE
Reindeer Chukchi and Siberian Eskimos, Russia	X	X	X	X		X	X	X						Tf, PGD, Hp, AcP PGM, AK	Sukernik et al. 1981:121-128
Ainu, Japan														Dental traits	Turner and Hanihara 1977: 13-24
Taiwanese populations														Dermatoglyphics	Chai 1971:369-376
Southeast Asian populations (various)														Hemoglobin E	Fix 1981:433-442
Pacific Populations (various)														HLA	Serjeantson et al. 1982:904-918
Papua New Guinea														Dermatoglyphics	Froelich and Giles 1981a:73-91
Papua New Guinea														Dermatoglyphics	Froelich and Giles 1981b:93-106
58 South American Indian populations (various locations)	X	X	X	X		X	X	X	X						Salzano 1957:555-580
North and South American native populations (various)														Hpl	Giblet and Best 1961:1300-1301
Six Central and six South American populations (various)		X	X	X			X			X					Roisenberg and Morton 1970:373-376
Survey of twenty-one North American populations	X	X	X	X		X	X								Spuhler 1972:72-95

Appendix 1 - Genetics Literature Survey - Table 11

GROUP(S) SURVEYED	ABO	MNSs	Rh	Fy^a	Le^a	Di^a	P	K	Lu^a	Jk^a	Js^a	Be^a	Wr^a	Other	REFERENCE
Survey of over forty Circum-Pacific peoples	X	X	X	X		X	X	X		X				Leuko-cyte gps & serum enzymes	Kirk 1979:211-237
Survey of New World populations														Serum album. Gm	Lampl and Blumberg 1979: 107-123
Peruvian Indians, Alaskan Eskimo, and eight East Asian populations														Gc hapto-globins	Matsumoto et al. 1980:505-508
Survey of east Siberian, Alaskan, Canadian, and Greenland Eskimo populations	X	X	X												Crawford and Enciso 1982:51-91
Survey of North American Native populations	X	X	X	X		X	X								O'Rourke et al. 1985:241-250
Survey of North and South American Native populations														Dermato-glyphics	Hoff et al. 1981: 455-461
Survey of North American Native populations														Cerumen types	Petrakis et al. 1967:1192-1193
Survey of North and South American Native populations														Cerumen types	Petrakis 1969: 1080-1081
Survey of East Asian and North and South American populations														Cerumen types	Petrakis et al. 1971:141-144
Survey of North and South American Native populations														Anthro-pometry	Newman 1953:311-327

Appendix 1 - Genetics Literature Survey - Table 1j

GROUP(S) SURVEYED	ABO	MNSs	Rh	Fy^a	Le^a	Di^a	P	K	Lu^a	Jk^a	Js^a	Be^a	Wr^a	Other	REFERENCE
Survey of selected East Asian, North and South American populations														PTC testing	Allison and Blumberg 1959: 352-359

NOTE: The organization of this table is based generally on geographical regions, date of publication, and trait types. The table incorporates blood group data with a variety of other genetic data listed in the last column. References are keyed by author, date, and exact page numbers. This variation on standard referencing was adopted because in a number of cases the senior author produced a number of articles in conjunction with the same, and also sometimes different groups of other authors during the same year. Serological gene systems are abbreviated throughout this table following standard abbreviations used in the field (cf. Roychoudhury and Nei 1988), and as used by the cited authors. This table is not meant to be exhaustive in any way. The data presented in this table are meant to represent a sampling of the published literature (primarily drawn from standard sources such as the American Journal of Physical Anthropology) covering approximately the last forty years. The intent of this survey is to establish a base line showing the amount and types of published genetic information available for further research.

Appendix 1 - Genetic Survey - Table 2a - ABO Blood Group

Location/ Group Name	1	N	Phenotypic Frequencies[2]				Gene Frequencies[3]			Reference
			A	B	O	AB	A	B	O	
Argentina:										
MAPUCHE	-	178	.0730	.0618	.8652	.0000	.037	.032	.931	Matson et al. (1969:61-84)
TOBA	0	51	.0000	.0000	1.0000	.0000	.000	.000	1.000	(Ibid.)
CHULUPI	-	96	.0000	.0104	.9896	.0000	.000	.005	.995	(Ibid.)
CHOROTI	0	142	.0000	.0000	1.0000	.0000	.000	.000	1.000	(Ibid.)
CHANES	-	120	.0083	.0083	.9834	.0000	.004	.004	.992	(Ibid.)
CHIRIGUANO	-	90	.1000	.0000	.9000	.0000	.049	.000	.948	(Ibid.)
MATACO	0	230	.0000	.0000	1.0000	.0000	.000	.000	1.000	(Ibid.)
DIAGUITA	-	38	.2631	.0000	.7369	.0000	.142	.000	.858	(Ibid.)
Chile:										
ALACALUF Is. Wellington	-	44	.0682	.0000	.9318	.0000	.035	.000	.965	Matson et al. (1968:157-184)
MAPUCHE Lonquimay	-	141	.0709	.0071	.9220	.0000	.036	.004	.960	(Ibid.)
ATACAMENOS	+	80	.0375	.0500	.9125	.0000	.019	.026	.955	(Ibid.)
ATACAMENOS	-	182	.1100	.0050	.8850	.0000	.8565	.0027	.9407	Rothhammer et al. (1984:243-247)
MAPUCHE	+	258	.1356	.0697	.7945	.0000	.0701	.3560	.9939	Sandoval & Henckel (1954:324-329)

Appendix 1 - Genetic Survey - Table 2b (Continued) - ABO Blood Group

Location/ Group Name	1	N	Phenotypic Frequencies[2] A	B	O	AB	Gene Frequencies[3] A	B	O	Reference
Paraguay:										
GUAYAKI	0	62	.0000	.0000	1.0000	.0000	.000	.000	1.000	Brown et al. (1974:317-344)
TOBA	0	14	.0000	.0000	1.0000	.0000	.000	.000	1.000	(Ibid.)
SANAPANA	0	104	.0000	.0000	1.0000	.0000	.000	.000	1.000	(Ibid.)
LENGUA	0	50	.0000	.0000	1.0000	.0000	.000	.000	1.000	(Ibid.)
CHULUPI	0	86	.0000	.0000	1.0000	.0000	.000	.000	1.000	(Ibid.)
CHEROTI	0	31	.0000	.0000	1.0000	.0000	.000	.000	1.000	(Ibid.)
AYORE	0	73	.0000	.0000	1.0000	.0000	.000	.000	1.000	(Ibid.)
GUARAYU	0	15	.0000	.0000	1.0000	.0000	.000	.000	1.000	(Ibid.)
TAPIETE	0	19	.0000	.0000	1.0000	.0000	.000	.000	1.000	(Ibid.)
CHAMACOCO	-	88	.0114	.0114	.9772	.0000	.006	.005	.989	Matson et al. (1968:81-98)
MORO	0	36	.0000	.0000	1.0000	.0000	.000	.000	1.000	(Ibid.)
CHULUPI	0	85	.0000	.0000	1.0000	.0000	.000	.000	1.000	(Ibid.)
LENGUA	0	207	.0000	.0000	1.0000	.0000	.000	.000	1.000	(Ibid.)
TOBA	0	100	.0000	.0000	1.0000	.0000	.000	.000	1.000	(Ibid.)
YAM LENGUA	0	20	.0000	.0000	1.0000	.0000	.000	.000	1.000	(Ibid.)
GUAYAKI	0	51	.0000	.0000	1.0000	.0000	.000	.000	1.000	(Ibid.)

Appendix 1 - Genetic Survey - Table 2c (Continued) - ABO Blood Group

Location/ Group Name	1	N	Phenotypic Frequencies[2]				Gene Frequencies[3]			Reference
			A	B	O	AB	A	B	O	
Bolivia:										
AYMARA	-	503	.0199	.0040	.9761	.0000	.010	.002	.988	Matson et al. (1966:13-34)
CHAMA	-	30	.0000	.0000	1.0000	.0000	.000	.000	1.000	(Ibid.)
TACANA	-	11	.1818	.0000	.8182	.0000	.096	.000	.904	(Ibid.)
CHACOBO	-	14	.0000	.0000	1.0000	.0000	.000	.000	1.000	(Ibid.)
ITONAMA	-	109	.1101	.0000	.8899	.0000	.057	.000	.943	(Ibid.)
MORE	-	67	.0746	.0000	.9254	.0000	.038	.000	.962	(Ibid.)
SIRONO	-	27	.0000	.0000	1.0000	.0000	.000	.000	1.000	(Ibid.)
Peru:										
PIRO	0	90	.0000	.0000	1.0000	.0000	.000	.000	1.000	Matson et al. (1966:325-350)
CHAMPA	0	89	.0000	.0000	1.0000	.0000	.000	.000	1.000	(Ibid.)
SHIPIBO	0	142	.0000	.0067	.9330	.0000	.000	.003	.997	(Ibid.)
ISCONAHUA	0	14	.0000	.0000	1.0000	.0000	.000	.000	1.000	(Ibid.)
AGUARUNA	0	151	.0000	.0000	1.0000	.0000	.000	.000	1.000	(Ibid.)
TICUNA	0	122	.0000	.0000	1.0000	.0000	.000	.000	1.000	(Ibid.)
YAGUA	0	9	.0000	.0000	1.0000	.0000	.000	.000	1.000	(Ibid.)

Appendix 1 - Genetic Survey - Table 2d (Continued) - ABO Blood Group

Location/ Group Name	1	N	Phenotypic Frequencies[2] A	 B	 O	 AB	Gene Frequencies[3] A	 B	 O	Reference
Peru (continued):										
AYMARA	0	93	.0000	.0000	1.0000	.0000	.000	.000	1.000	Matson et al. (1966:325-350)
QUECHUA	-	181	.0830	.0165	.9005	.0000	.043	.006	.949	(Ibid.)
AYMARA	-	58	.0000	.0000	1.0000	.0000	.000	.000	1.000	Best et al. (1962:321-329)
QUECHUA	-	119	.0336	.0504	.9160	.0000	.0225	.0310	.9465	(Ibid.)
Ecuador:										
QUECHUA Central Plateau	-	372	.0376	.0108	.9516	.0000	.019	.005	.976	Matson et al. (1966:51-70)
QUECHUA North Plateau	-	232	.0216	.0388	.9353	.0043	.013	.020	.967	(Ibid.)
QUECHUA South Plateau	-	54	.0556	.0556	.8889	.0000	.028	.028	.944	(Ibid.)
COLORADO	0	36	.0000	.0000	1.0000	.0000	.000	.000	1.000	(Ibid.)
JIVARO	0	233	.0000	.0000	1.0000	.0000	.000	.000	1.000	(Ibid.)
CAYAPA	-	244	.0000	.0164	.9836	.0000	.000	.008	.992	(Ibid.)
SECOYA	0	48	.0000	.0000	1.0000	.0000	.000	.000	1.000	(Ibid.)

Appendix 1 - Genetic Survey - Table 2a (Continued) - ABO Blood Group

Location/ Group Name	1	N	Phenotypic Frequencies[2] A	B	O	AB	Gene Frequencies[3] A	B	O	Reference
Brazil:										
PARAMITERI Northern Brazil	0	9	.0000	.0000	1.0000	.0000	.000	.000	1.000	Matson et al. (1968:303-330)
CACARAPAI Northern Brazil	0	9	.0000	.0000	1.0000	.0000	.000	.000	1.000	(Ibid.)
ASSURINI Northern Brazil	0	21	.0000	.0000	1.0000	.0000	.000	.000	1.000	(Ibid.)
GALIBI Northern Brazil	0	38	.0000	.0000	1.0000	.0000	.000	.000	1.000	Matson et al. (1968:303-330)
UAPIXANA Northern Brazil	0	10	.0000	.0000	1.0000	.0000	.000	.000	1.000	(Ibid.)
MACUXI Northern Brazil	-	261	.0038	.0038	.9924	.0000	.002	.002	.996	(Ibid.)
UAICA Northern Brazil	0	48	.0000	.0000	1.0000	.0000	.000	.000	1.000	(Ibid.)
XIRIXANO Northern Brazil	0	27	.0000	.0000	1.0000	.0000	.000	.000	1.000	(Ibid.)
BANIWAS Northwest Brazil	-	514	.0000	.0000	1.0000	.0000	.000	.000	1.000	Salzano et al. (1986:3-14)
GUARITA CAINGANG South. Brazil	-	88	.0113	.0114	.9773	.0000	.0057	.0057	.9886	Salzano (1961:391-404)
NONOAI CAINGANG South. Brazil	-	122	.0082	.0092	.9836	.0000	.0048	.0048	.9904	(Ibid.)

Appendix 1 - Genetic Survey - Table 2f (Continued) - ABO Blood Group

Location/ Group Name	1	N	Phenotypic Frequencies[2]				Gene Frequencies[3]			Reference
			A	B	O	AB	A	B	O	
Brazil (continued):										
LIGEIRO CAINGANG South. Brazil	-	65	.0154	.0000	.9846	.0000	.0104	.0000	.9896	Salzano (1961;391-404)
PARAKANA Northern Brazil	0	100	.0000	.0000	1.0000	.0000	.000	.000	1.000	Black et al. (1980;118-132)
CAYAPO Central Brazil	0	305	.0000	.0000	1.0000	.0000	.000	.000	1.000	Salzano et al. (1977;337-348)
KRAHO Central Brazil	0	190	.0000	.0000	1.0000	.0000	.000	.000	1.000	(Ibid.)
KAMAIURA/ KALAPALO Matto Grosso	0	73	.0000	.0000	1.0000	.0000	.000	.000	1.000	Pantin and Junqueira (1952; 385-405)
XAVANTE Matto Grosso	-	460	.0000	.0000	1.0000	.0000	.000	.000	1.000	Gershowitz et al. (1967;502-537)
Brazil/British Guiana:										
MACUSHI CARIBS	0	119	.0000	.0000	1.0000	.0000	.000	.000	1.000	Layrisse et al. (1963b:140-166)
British Guiana:										
ACAWAIO CARIBS	0	90	.0000	.0000	1.0000	.0000	.000	.000	1.000	Layrisse et al. (1963b:140-166)

Appendix 1 - Genetic Survey - Table 2g (Continued) - ABO Blood Group

Location/ Group Name	1	N	[2] Phenotypic Frequencies A	B	O	AB	Gene Frequencies[3] A	B	O	Reference
Venezuela:										
SOTO CARIBS	0	86	.0000	.0000	1.0000	.0000	.000	.000	1.000	Layrisse et al. (1963b:140-166)
ACAWAIO-MACUSHI CARIBS	-	209	.0000	.0000	1.0000	.0000	.000	.000	1.000	(Ibid.)
PANARE CARIBS	0	33	.0000	.0000	1.0000	.0000	.000	.000	1.000	(Ibid.)
YUPA CARIBS	-	176	.0000	.0000	1.0000	.0000	.000	.000	1.000	(Ibid.)
PEMON CARIBS	-	287	.0000	.0000	1.0000	.0000	.000	.000	1.000	(Ibid.)
WARRAU	0	153	.0000	.0000	1.0000	.0000	.000	.000	1.000	Layrisse et al. (1963a:36-55)
Colombia:										
NOANAMA	0	168	.0000	.0000	1.0000	.0000	.000	.000	1.000	Kirk et al. (1974:301-316)
COFAN	-	78	.0000	.0000	1.0000	.0000	.000	.000	1.000	(Ibid.)
IGANO	-	51	.0588	.0000	.9412	.0000	.0299	.0000	.9701	(Ibid.)
SIONA	-	64	.0469	.0312	.9219	.0000	.0237	.0158	.9605	(Ibid.)
ICA	-	114	.0263	.0088	.9649	.0000	.0134	.0044	.9822	Layrisse et al. (1963a:36-55)
PAEZ	-	104	.0673	.0673	.8654	.0000	.0355	.0355	.9291	(Ibid.)

Appendix 1 - Genetic Survey - Table 2h (Continued) - ABO Blood Group

Location/ Group Name	1	N	Phenotypic Frequencies[2]				Gene Frequencies[3]			Reference
			A	B	O	AB	A	B	O	
Colombia (continued):										
TUNEBO	-	100	.0100	.0100	.9800	.0000	.0055	.0055	.9900	Layrisse et al. (1963a:36-55)
Panama:										
GUAYAMI	-	240	.0000	.0000	1.0000	.0000	.000	.000	1.000	Matson & Swanson (1965a:413-426)
SAN BLAS CUNA	-	388	.0000	.0000	1.0000	.0000	.000	.000	1.000	(Ibid.)
CHOCO	-	80	.0000	.0000	1.0000	.0000	.000	.000	1.000	(Ibid.)
Costa Rica:										
BORUCA	-	56	.0179	.0357	.9464	.0000	.0092	.0182	.9728	Matson & Swanson (1965b:107-122)
BRIBRI	0	50	.0000	.0000	1.0000	.0000	.000	.000	1.000	Matson & Swanson (1965b:107-122)
CABECAR	0	62	.0000	.0000	1.0000	.0000	.000	.000	1.000	(Ibid.)
TERRABAS	0	40	.0000	.0000	1.0000	.0000	.000	.000	1.000	(Ibid.)
Nicaragua:										
CHOROTEGA	-	77	.0649	.0130	.9221	.0000	.0330	.0065	.9605	Matson & Swanson (1963a:545-559)
MISKITO	-	150	.0867	.0133	.9000	.0000	.0443	.0067	.9490	(Ibid.)

Appendix 1 - Genetic Survey - Table 2i (Continued) - ABO Blood Group

Location/ Group Name	1	N	Phenotypic Frequencies[2] A	B	O	AB	Gene Frequencies[3] A	B	O	Reference
Nicaragua (continued):										
RAMA	-	38	.0263	.0000	.9737	.0000	.0132	.0000	.9868	Matson & Swanson (1963a:545-559)
SUMO	0	103	.0000	.0000	1.0000	.0000	.0000	.0000	1.0000	(Ibid.)
SUBTIABA	-	29	.0690	.0690	.8621	.0000	.0351	.0351	.9298	(Ibid.)
Honduras:										
JICAQUE	+	194	.1804	.1340	.6495	.0361	.1143	.0886	.7971	Matson & Swanson (1963b:319-334)
LENCA	-	152	.0658	.0263	.9079	.0000	.0335	.0132	.9533	(Ibid.)
PAYA	-	53	.0566	.0000	.9434	.0000	.0287	.0000	.9713	(Ibid.)
Guatemala:										
CAKCHIQUEL MAYA	-	150	.0267	.0267	.9466	.0000	.0134	.0134	.9732	Matson & Swanson (1963d:1-14)
KEKCHI MAYA	-	162	.0309	.0123	.9506	.0062	.0217	.0124	.9659	Matson & Swanson (1963d:1-14)
MAM MAYA	-	116	.0345	.0000	.9655	.0000	.0342	.0000	.9658	(Ibid.)
QUICHE MAYA	-	203	.0640	.0099	.9261	.0000	.0326	.0049	.9624	(Ibid.)

Appendix 1 - Genetic Survey - Table 2j (Continued) - ABO Blood Group

Location/ Group Name	1	N	Phenotypic Frequencies[2] A	B	O	AB	Gene Frequencies[3] A	B	O	Reference
Belize:										
MAYA AT SAN ANTONIO	-	230	.0162	.0564	.9274	.0000	.0080	.0290	.9630	Matson & Swanson (1964:271-284)
KEKCHI MAYA	-	117	.0168	.0000	.9832	.0000	.0085	.0000	.9915	(Ibid.)
Mexico:										
CHOL	-	152	.1053	.0066	.8882	.0000	.054	.004	.942	Cordova et al. (1967:55-66)
CHONTAL	-	101	.0990	.0297	.8713	.0000	.051	.016	.933	(Ibid.)
TOTONAC	-	79	.1139	.0000	.8861	.0000	.059	.000	.941	(Ibid.)
HUASTEC	-	76	.0000	.0526	.9474	.0000	.000	.027	.973	(Ibid.)
MIXE	-	32	.0313	.0313	.9375	.0000	.016	.016	.968	(Ibid.)
MAZATEC	-	136	.0588	.0074	.9338	.0000	.030	.004	.966	(Ibid.)
ZAPOTEC	-	106	.0660	.0283	.9057	.0000	.034	.015	.951	(Ibid.)
MIXTEC	-	40	.1750	.1000	.7250	.0000	.135	.023	.842	(Ibid.)
CHINANTEC	-	21	.0000	.0000	1.0000	.0000	.000	.000	1.000	(Ibid.)
NAHUA Veracruz	-	141	.0922	.0000	.9007	.0071	.047	.004	.949	Cordova et al. (1967:55-66)
CORA	-	96	.0313	.0000	.9688	.0000	.016	.000	.984	(Ibid.)
HUICHOL	-	71	.0000	.0000	1.0000	.0000	.000	.000	1.000	(Ibid.)

Appendix 1 - Genetic Survey - Table 2k (Continued) - ABO Blood Group

Location/ Group Name	1	N	[2] Phenotypic Frequencies A	B	O	AB	Gene Frequencies[3] A	B	O	Reference
Mexico (continued):										
NAHUA Puebla	-	42	.0952	.0000	.9048	.0000	.049	.000	.951	Cordova et al. (1967:55-66)
TLAXCALTECAN Saltillo	+	302	.2119	.1060	.6755	.0066	.3332	.0581	.8596	Crawford et al. (1979:401-412)
TLAXCALA San Pablo del Monte	-	257	.0970	.0080	.8950	.0000	.050	.004	.946	Crawford et al. (1974:251-268)
TLAXCALA (City of Tlaxcala)	+	138	.1960	.0650	.7170	.0220	.115	.044	.841	(Ibid.)
CHOL (MAYA)	-	15	.1333	.0000	.8667	.0000	.0691	.0000	.9309	Matson et al. (1963d:1-14)
ITZA (MAYA)	-	67	.0896	.0000	.8657	.0447	.0688	.0223	.9087	(Ibid.)
LACANDON MAYA	-	61	.0000	.0000	1.0000	.0000	.0000	.0000	1.0000	(Ibid.)
LACANDON MAYA	-	33	.0303	.0000	.9697	.0000	.0153	.0000	.9847	(Ibid.)
TZELTAL MAYA	-	111	.0090	.0000	.9910	.0000	.0044	.0000	.9956	(Ibid.)
TZOTZIL MAYA	-	80	.0125	.0000	.9875	.0000	.0063	.0000	.9937	(Ibid.)
TZOTZIL MAYA	-	91	.0000	.0220	.9780	.0000	.0000	.0111	.9889	(Ibid.)
Southeastern U. S. A.:										
CHEROKEE	-	166	.0361	.0181	.9458	.0000	.0182	.0091	.9727	Pollitzer et al. (1962:33-43)

Appendix 1 - Genetic Survey - Table 21 (Continued) - ABO Blood Group

Location/ Group Name	1	N	Phenotypic Frequencies[2] A	B	O	AB	Gene Frequencies[3] A	B	O	Reference
New Mexico U. S. A.:										
ZUNI	-	662	.0226	.1073	.8701	.0000	.011	.055	.933	Workman et al. (1974:119-132)
California U. S. A.:										
DIEGUENO	-	58	.0172	.0172	.9655	.0000	.0087	.0087	.9826	Pantin & Kallsen (1953:91-96)
Utah U. S. A.:										
UTE	-	104	.0192	.0000	.9808	.0000	.010	.000	.990	Matson & Piper (1947:357-368)
Minnesota U. S. A.:										
CHIPPEWA	-	161	.1240	.0000	.8760	.0000	.064	.000	.936	Matson et al. (1954:413-426)
Washington State U. S. A.:										
SWINOMISH	-	102	.1176	.0000	.8824	.0000	.061	.000	.939	Hulse (1957:235-246)
YAKIMA	-	95	.2526	.0000	.7474	.0000	.124	.000	.876	(Ibid.)
Washington State/British Columbia, Canada:										
OKANOGAN	-	72	.1944	.0000	.8056	.0000	.102	.000	.898	Hulse (1957:235-246)

Appendix 1 - Genetic Survey - Table 2m (Continued) - ABO Blood Group

Location/ Group Name	1	N	Phenotypic Frequencies[2] A	B	O	AB	Gene Frequencies[3] A	B	O	Reference
Alberta, Canada:										
BLOOD	-	241	.8133	.0083	.1660	.0124	.5824	.0104	.4072	Chown & Lewis (1953:369-383)
BLACKFOOT	-	39	.8462	.0000	.1538	.0000	.6078	.0000	.3922	(Ibid.)
Alaska, U. S. A.:										
ESKIMO Savoonga	-	169	.4320	.1479	.3905	.0296	.2683	.0938	.6379	Crawford et al. (1981:167-185)
ESKIMO Gambell	-	80	.4500	.1250	.3125	.1125	.3344	.1252	.5404	(Ibid.)
ESKIMO Wales	-	67	.4328	.0597	.4179	.0895	.3051	.0764	.6185	(Ibid.)
ESKIMO King Island	-	54	.6300	.1480	.1480	.0740	.4650	.1205	.4145	(Ibid.)
ESKIMO Savoonga	-	149	.4027	.1611	.4027	.0336	.251	.103	.646	Ferrell et al. (1981:351-358)
ESKIMO Gambell	-	72	.2500	.2778	.3889	.0833	.183	.200	.617	(Ibid.)
ESKIMO Bethel	-	341	.4487	.1173	.3695	.0645	.299	.095	.607	Matson & Roberts (1949:109-122)
New World:										

Total number of samples listed: 147

Total for N all groups listed: 17,481

Appendix 1 - Genetic Survey - Table 2n (Continued) - ABO Blood Group

Location/ Group Name	1	N	Phenotypic Frequencies[2] A	B	O	AB	Gene Frequencies[3] A	B	O	Reference
Non-American Samples:										
CAUCASIAN (SPAIN)	-	Cited for [4]comparative purposes only					.278	.061	.660	Crawford et al. (1974:251-268)
CHUKCHI (Various)	N/A	1,066	.3068	.2289	.4212	.0432	.195	.149	.656	Sukernik et al. (1981:121-128)
ESKIMO (Northeastern Siberia)	N/A	102	.2255	.2941	.4412	.0392	.143	.184	.672	(Ibid.)

Notes:

1 This column gives an estimate by the author(s) (either explicit or implicit) of the ammount of non-Amerindian genetic admixture present in the sample. 0 = no known or suspected admixture; - = some admixture in varying smaller amounts; + = clear and significant levels of admixture; and N/A = not applicable.

2 Given variability in reporting of A_1 and A_2 data, all variants were grouped and the data reported herein was recalculated accordingly.

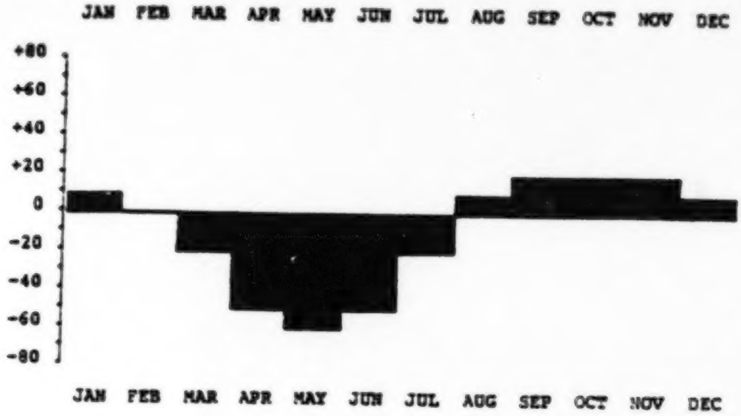
3 Gene frequencies were recalculated as necessary (see note 2) and in cases of obvious typographical errors in the original published data. Recalculations were based on Berenstein's correction formula.

4 This survey was limited to primary sources. For additional similar, published surveys, see Salzano's (1957) survey of 58 South American groups (total N = 18,188); and Spuhler's (1972) survey of 21 North American populations (no total numbers of individuals given). These two surveys are largely non-overlaping with the data presented in the survey results given herein. See also the data presented in Mourant et al. (1976), and Roychoudhury and Nei (1988) for broader, world-wide surveys.

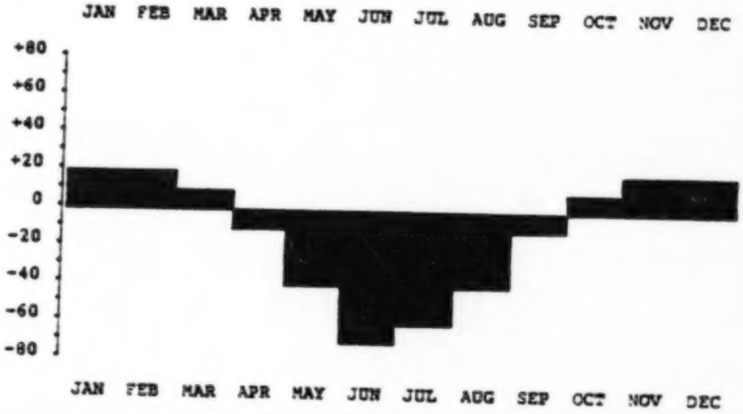
Appendix 2

LONG-TERM VARIATIONS IN INSOLATION

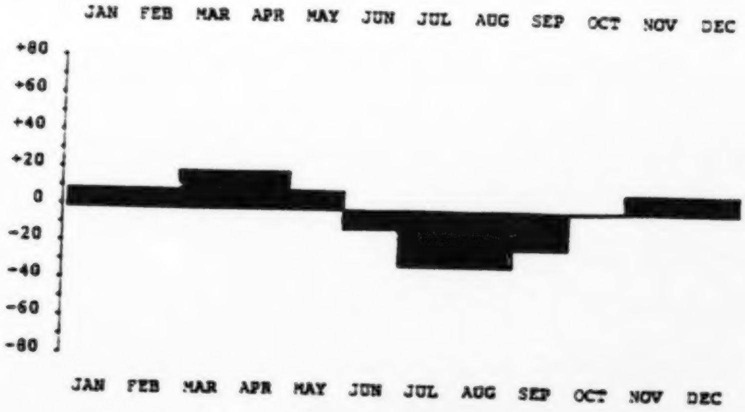
75 Ka (- 110) [0.00]



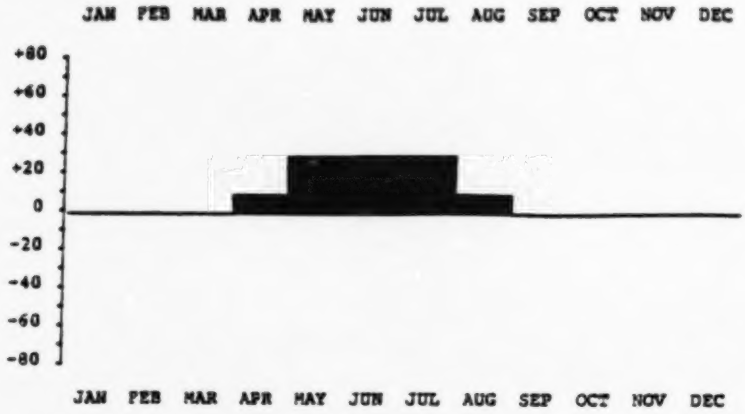
70 Ka (- 130) [- 0.37]



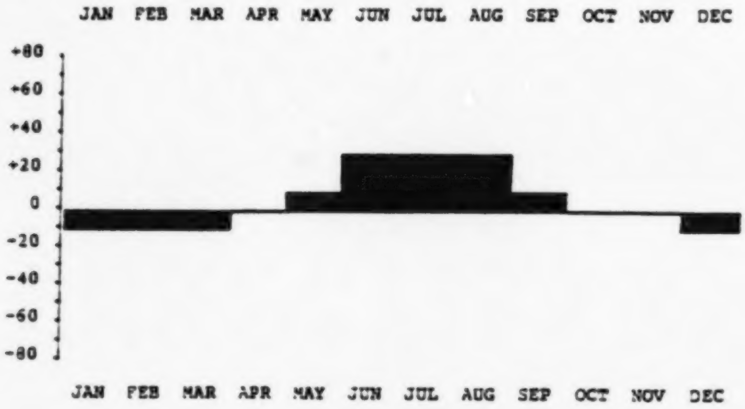
65 Ka (00) [- 0.48]



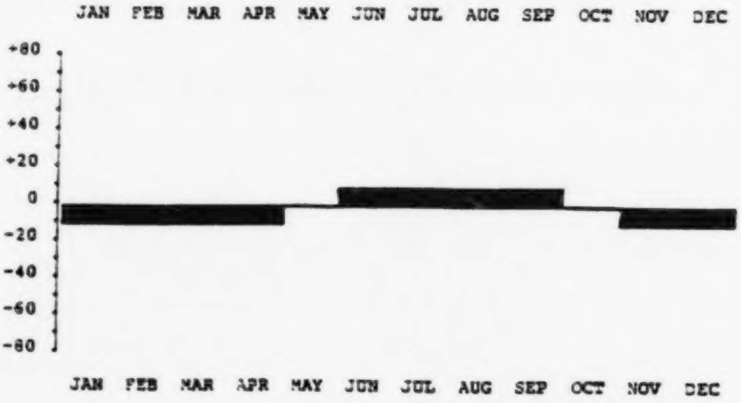
60 Ka (+ 110) [- 0.41]



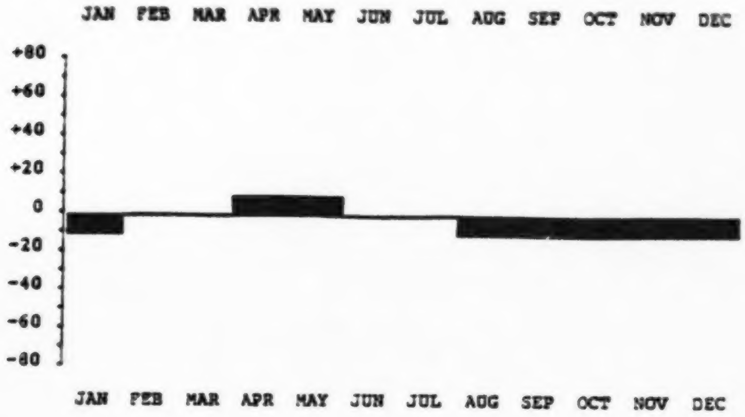
55 Ka (+ 70) [- 0.16]



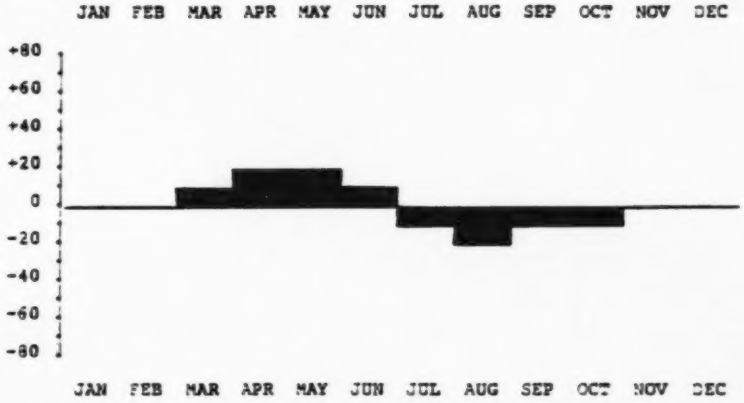
50 Ka (- 20) [- 0.17]



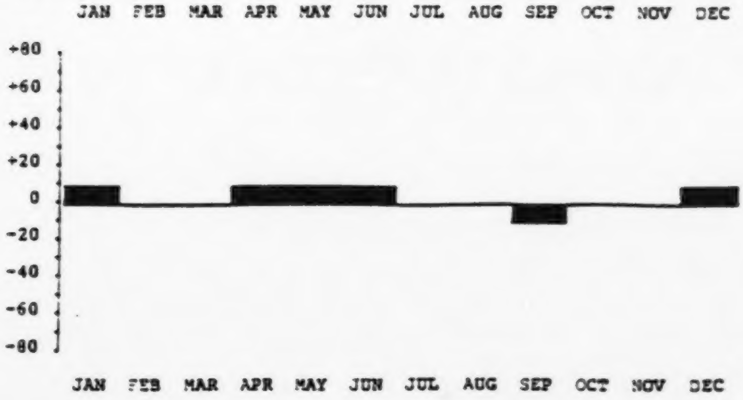
45 Ka (- 40) [- 0.37]



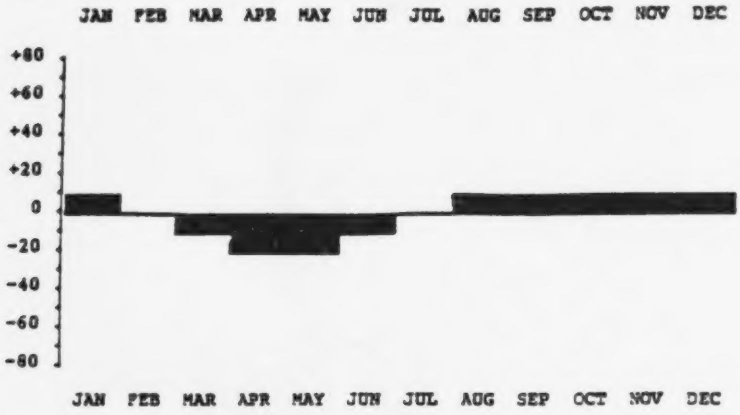
40 Ka (+ 10) [- 0.43]



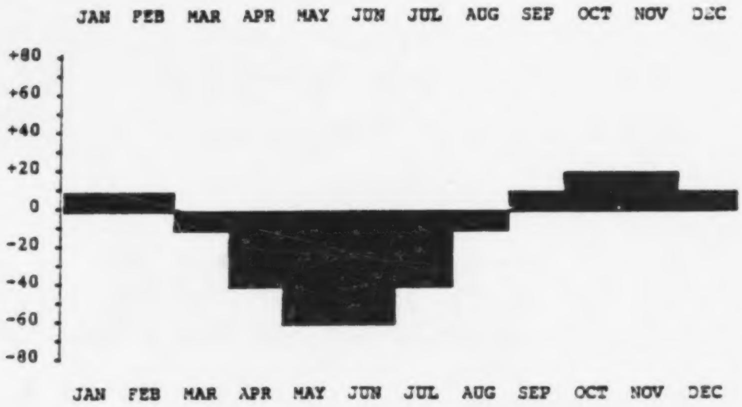
35 Ka (+ 40) [- 0.45]



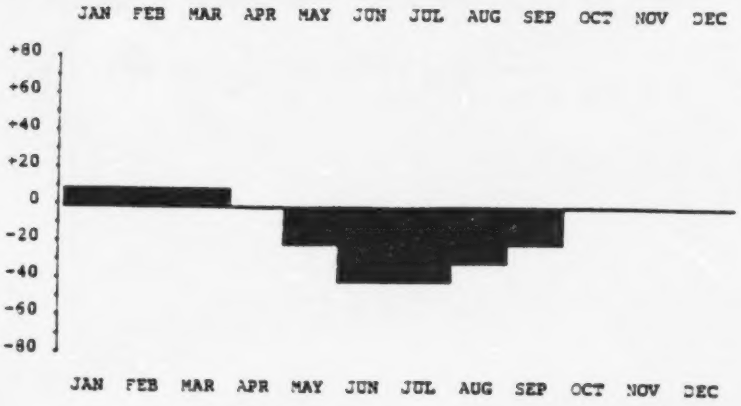
30 Ka (00) [- 0.42]



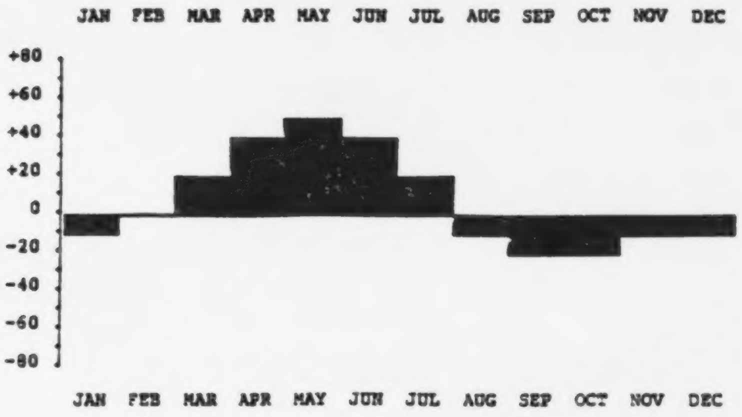
25 Ka (- 140) [- 0.49]



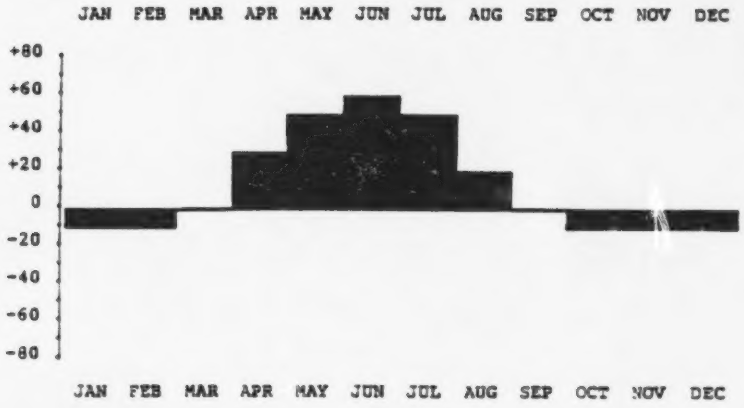
20 Ka (- 120) [- 0.76]



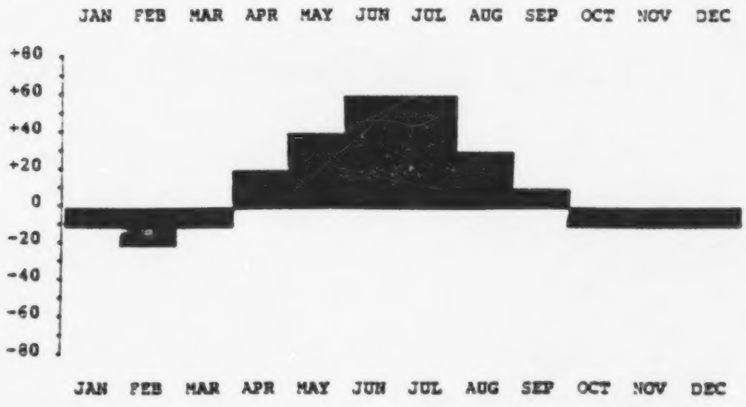
15 Ka (+ 90) [- 0.62]

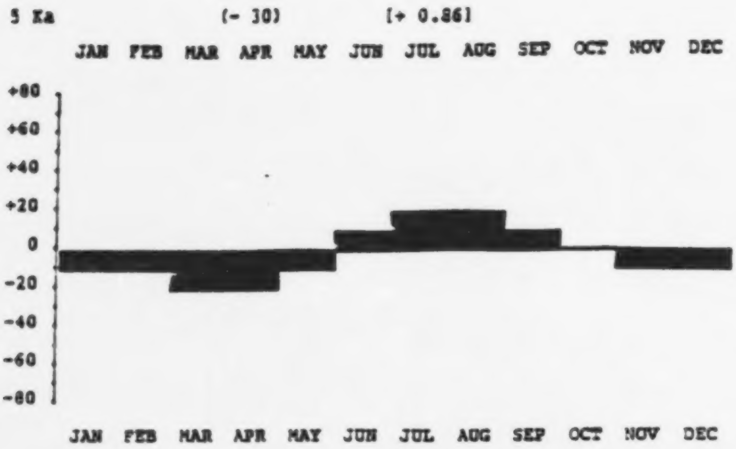


12 Ka (+ 160) [- 0.06]



10 Ka (+ 150) [+ 0.31]





Orbital Forcing, Seasonality, Sea levels, and the

Oxygen Isotope Record Between 75 Ka and 5Ka BP

Each seasonal insolation contour is labeled at the top by the date in thousands of years BP, followed by the estimated global sea level relative to modern sea level, and the corresponding oxygen isotope value. Monthly insolation values have been rounded by increments of ten langley deviations from their average values over the last 500,000 years at 60 degrees north latitude. The data presented are based on Berger (1978, 1979); Bradley (1985); and Martinson et al. (1987).